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Power Relations in Three-Person Groups	<i>Theodore M. Mills</i>
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Role Conflict and Ambivalence in Leadership	<i>Melvin Seeman</i>
Voluntary Associations in a Mexican City	<i>Floyd Dotson</i>
Some Principles of Stratification	<i>Melvin M. Tumin</i>
Reply and Comment	<i>Kingsley Davis and Wilbert Moore</i>
Social Class Identification in the Urban Community	<i>Neal Gross</i>
Trends in Occupational Origins of Physicians	<i>Stuart Adams</i>
Message Diffusion in Controlled Experiments	<i>Stuart C. Dodd</i>
The Accuracy of Census Results	<i>Morris H. Hansen, William N. Hurwitz, and Leon Pritzker</i>
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Contents

Power Relations in Three-Person Groups.....	THEODORE M. MILLS	351
An Experimental Approach to the Study of Status Relations in Informal Groups.....	O. J. HARVEY	357
Some Functional Consequences of Primary Controls in Formal Work Organizations.....	EDWARD GROSS	368
Role Conflict and Ambivalence in Leadership....	MELVIN SEEMAN	373
A Note on Participation in Voluntary Associations in a Mexican City.....	FLOYD DOTSON	380
Some Principles of Stratification: A Critical Analysis.....	MELVIN M. TUMIN	387
Reply.....	KINGSLEY DAVIS	394
Comment.....	WILBERT MOORE	397
Social Class Identification in the Urban Community...	NEAL GROSS	398
Trends in Occupational Origins of Physicians.....	STUART ADAMS	404
Testing Message Diffusion in Controlled Experiments: Charting the Distance and Time Factors in the Interactance Hypothesis....	S. C. DODD	410
The Accuracy of Census Results.....	MORRIS H. HANSEN, WILLIAM N. HURWITZ, AND LEON PRITZKER	416
Marital Happiness of Parents and Their Children's Attitudes to Them.....	PAUL WALLIN AND HOWARD M. VOLLMER	424

OFFICIAL REPORTS

NEWS AND ANNOUNCEMENTS

BOOK REVIEWS

Pumpian-Mindlin: <i>Psychoanalysis as Science</i> . Robert E. L. Faris...	437
Asch: <i>Social Psychology</i> . G. E. Swanson.....	439
Ferguson: <i>Personality Measurement</i> . Harrison G. Gough.....	440
Barnett: <i>Innovation</i> . Stephen W. Reed.....	441
Kroeber and Kluckhohn: <i>Culture</i> . Joseph Bram.....	442
Nisbet: <i>The Quest for Community</i> . Arthur K. Davis.....	443
Mannheim: <i>Essays on the Sociology of Knowledge</i> . Bernard Barber	444
Knapp and Goodrich: <i>Origins of American Scientists</i> . Bernard Barber.....	445

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Seattle 5, Washington

Executive Office

New York University
Washington Square
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Toynbee: <i>The World and the West</i> . Harry Elmer Barnes.....	445
Cowell: <i>History, Civilization and Culture</i> . Harry Elmer Barnes....	445
Ulam: <i>Titoism and the Cominform</i> . D. A. Tomasic.....	447
North: <i>Kuomintang and Chinese Communist Elites</i> . Fenton Keyes..	448
Hoselitz: <i>The Progress of Underdeveloped Areas</i> . Simon Marcson..	449
Stamp: <i>Land for Tomorrow</i> . Carl C. Taylor.....	450
Hardin: <i>The Politics of Agriculture</i> . T. Lynn Smith.....	452
Zimmerman and Du Wors: <i>Graphic Regional Sociology</i> . Howard W. Beers.....	453
Bogue: <i>A Methodological Study of Migration</i> . Henry S. Shryock, Jr.	453
Queen and Carpenter: <i>The American City</i> . Harold W. Saunders...	455
Landis and Landis: <i>Readings in Marriage and the Family</i> . Charles Bowerman.....	456
Pilpel and Zavin: <i>Your Marriage and the Law</i> . Jessie Bernard....	456
Mariano: <i>A Psychoanalytic Lawyer Looks at Marriage and Divorce</i> . Jessie Bernard.....	456
Cabot and Kahl: <i>Human Relations</i> . L. J. Carr.....	457
Andrews: <i>The Case Method of Teaching Human Relations and Administration</i> . Donald E. Wray.....	458
Ellsworth: <i>Factory Folkways</i> . William Foote Whyte.....	459
Scott and Lynton: <i>Three Studies in Management</i> . Wilbert E. Moore	460
Vucinich: <i>Soviet Economic Institutions</i> . John S. Ellsworth, Jr....	460
Haveman: <i>De Ongeschoolde Arbeider</i> . Bart Landheer.....	461
Knowles: <i>Strikes</i> . Jack London.....	462
Wells: <i>Monopoly and Social Control</i> . Richard T. LaPiere.....	463
Schuler, et al: <i>Medical Public Relations</i> . Carl E. Hopkins.....	463
Furfey: <i>The Scope and Method of Sociology</i> . Ethel Shanas.....	464
Merton, et al: <i>The Focussed Interview</i> . Shirley A. Star.....	465
Freund: <i>Modern Elementary Statistics</i> . Julius A. Jahn.....	466
French: <i>An Approach to Measuring Results in Social Work</i> . Ernest Greenwood.....	467
Riecken: <i>The Volunteer Work Camp</i> . R. A. Schermerhorn.....	468
Meyer: <i>Social and Psychological Factors in Opiate Addiction</i> . W. W. Weaver.....	469

PUBLICATIONS RECEIVED

(Articles in the AMERICAN SOCIOLOGICAL REVIEW are indexed in the International Index to Periodicals and in the Weekly Bulletin of Public Affairs Information Service.)

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POWER RELATIONS IN THREE-PERSON GROUPS *

THEODORE M. MILLS

Harvard University

IN drawing his fundamental distinction between two-person groups and all groups of larger size, Simmel¹ called attention to certain characteristics of the three-person situation. It was important to him the way the position of the third person impinged upon the other two, whether this position be as mediator, as holder of the balance of power, or as constant disturber of the solidarity enjoyed by the other two. He described how a conflict between two could bring satisfaction and strength to a third, and how the sense of unity within a pair could be threatened by the mere presence of another person.

More recently, and on quite another plane, Von Neumann and Morgenstern² have made an important contribution by making room,

in their theory, for the three-person game and, in their solution, by allowing for a coalition between two parties to the game. Their model has a place within it for this alliance, whether it be collusion in the market place, a strong emotional tie, or simply a gentleman's agreement.

In this respect, Simmel and Von Neumann and Morgenstern share common ground. One assumes and the others plan for an elementary differentiating tendency in the threesome; namely, segregation into a *pair* and an *other*.

Whether or not this segregation generally occurs, and just what position the third party *does* take (or is given) in the face of conflict or alliance between the others, are empirical questions. They are precisely the sort of questions that are easier to investigate now than they were before recent developments in the observation of behavior in small groups.

Simmel's principle of segregation is the first of three questions examined in the paper. The second is the extent to which relationships are interdependent. The problem is whether the nature of one relationship determines to any appreciable degree the nature of the other relationships? The third follows naturally from these: when is it found that interdependence develops into a sharply differentiated and rigidly set power structure, and when is it found that relationships are in a state of fluctuation?

PROCEDURES

Observation Setting. The data consist of observations made of interaction in forty-eight three-person discussion sessions. Sub-

* This research was carried out at the Harvard Laboratory of Social Relations under contract AF33 (038)-12782, monitored by the Human Resources Research Institute, with data collected while the author held a Social Science Research Council fellowship under the supervision of Dr. Robert F. Bales. Permission is granted for reproduction, translation, publication and disposal in whole and in part by or for the United States Government. The author gratefully acknowledges the contributions, in the form of critical review of a preliminary draft, of Professors Talcott Parsons and Samuel A. Stouffer, Dr. Robert F. Bales, Dr. Andrew F. Henry, Dr. Duncan MacCrae and other members of the Harvard Laboratory, and of Mary E. Roseborough and Christoph Heineke in scoring the interaction.

¹ Kurt H. Wolff (translator and editor), *The Sociology of Georg Simmel*, Glencoe, Illinois: The Free Press, 1950, Chapters 2, 3, and 4.

² John Von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior*, Princeton, New Jersey: Princeton University Press, 1944, Chapter 5.

jects were student volunteers, recruited through the Harvard Student Employment Service. Groups were assembled in a room equipped with an adjacent observation room. Subjects had not previously interacted with one another, and there were no marked status differences between them. A group performed in two sessions, each lasting around thirty minutes.

Each group was asked to create, from three pictures selected from the T.A.T. series, a single dramatic story upon which they all agreed. In the experimental setting there was a minimum of restraints; no limit was placed upon the kind of story, or its content, or upon what member should play what role in its telling.

Collection of Data. Each act was scored in sequence according to Bales' method of interaction process analysis.³ This score shows, first, who initiates the act and to whom it is directed. In addition, it indicates the relevance of the act either to the solution of the problem confronting the group or to the state of integration of the group. Acts classified as relevant primarily to the group problem (categories four through nine) are for present purposes combined and called "contributions." Positive acts (categories one through three) directed specifically to others in the group are called acts of "support"; negative acts (categories ten through twelve) directed specifically to others are called acts of "non-support." In this manner interaction is divided into items offered to the group's solution and into positive or negative responses to what is offered.

A summary tabulation of scores for the entire discussion provides two important sets of data; first, the relative number of contributions made by each member, and, second, the exchange of supportive and non-supportive responses between members.

Ordering the Data. These data are ordered in a matrix illustrated in Table 1. The member in the group who is highest in contributions is assigned, as initiator, to the first row of the matrix, and, as recipient, to the first column. Others are assigned to succeeding rows and columns according to the rank order of their contributions. Within matrix cells are

placed the rates of support between members. In the illustration, the rate from the most active member to the medium active member is 22.1, and this may be taken to read roughly that the former overtly supports 22 per cent of the latter's contributions. The rate in the adjacent cell indicates that the most active member supports around three per cent of the least active member's contributions.⁴ Rates of total support *output* are recorded to the

⁴ More precisely, the rates indicate the *preponderance* of supportive versus non-supportive acts, for the calculation of the rate of support takes into account both classes of acts. Support is assumed to range from positive (where supportive acts outnumber non-supportive) through zero (where supportive and non-supportive acts are equal) to negative (where non-supportive outnumber supportive acts). Details in the calculation of rates to specific persons in the group and of total output and intake rates are given in the following formulae:

- (A) The rate at which member 1 supports member 2 (RS_{12}) is given, for example, by the equation:

$$RS_{12} = 100 \cdot \frac{A_{12} - D_{12}}{B_2 + C_2}$$

Where: A_{12} refers to the frequency of supportive acts initiated by member 1 and directed to member 2.

D_{12} refers to the frequency of non-supportive acts initiated by member 1 and directed to member 2.

B_2 and C_2 combined refer to the frequency of "contributions" (as defined above in the text) initiated by member 2 regardless of the recipients of the acts.

- (B) The rate of total support output of member 1 ($RTSO_1$) is given by the following equation:

$$RTSO_1 = 100 \cdot \frac{(A_{12} + A_{13} + \dots A_{1n}) - (D_{12} + D_{13} + \dots D_{1n})}{(B_2 + B_3 + \dots B_n) + (C_2 + C_3 + \dots C_n)}$$

Where the letter symbols have the same meaning in respect to classes of acts as in the equation above, and where the subscripts, as above, indicate the initiator and the recipient in that order. A single subscript indicates that all initiations, regardless of recipient, are included.

- (C) The rate of total support intake of member 1 ($RSTI_1$) is given by the following equation:

$$RSTI_1 = 100 \cdot \frac{(A_{21} + A_{31} + \dots A_{n1}) - (D_{21} + D_{31} + \dots D_{n1})}{B_1 + C_1}$$

Where letter symbols and subscripts have the same reference as in the above equations. Note that the use of the letters corresponds to Bales' designation, *op. cit.*, p. 9.

³ Robert F. Bales, *Interaction Process Analysis*, Cambridge, Massachusetts: Addison-Wesley Press, 1950.

TABLE 1. ILLUSTRATION OF MATRIX, SHOWING RATES OF SUPPORT ACCORDING TO RANK ORDER OF CONTRIBUTIONS

(Experimental Group No. 40; Total Acts: 493)

As Initiator	As Recipient			Rate of Total Support Output
	Most Active	Medium Active	Least Active	
Most Active	22.1	3.2	15.0
Medium Active	24.2	11.1	20.9
Least Active	.5	1.08
Rate of Total Support Received	24.8	23.0	14.3

right of the matrix; rates of total support intake, below the matrix.

These procedures result in a standardized matrix presenting two components that are taken together as a first approximation to the definition of the power position of a member; first, the relative number of contributions, and second, the relative frequency of support others give to the author of these contributions. It is inferred, in other words, that a high rate of participation coupled with high support intake means that a member is in a relatively strong position, while low participation coupled with a low rate of support means a relatively weak position.

Moreover, for any pair of members, the magnitude of the rates of support are taken as manifestations of the nature of the relationship existing within that pair. Thus in the group illustrated in Table 1, it may be said, tentatively at least, that the two more active members are in a solidary alliance, the most and the least in conflict, and the medium and least active in an unbalanced, non-reciprocal relationship. The most active member is in the strongest power position, the least active, in the weakest.

A matrix, like the one illustrated, is calculated for each session, and these matrices form the base for further steps in ordering the data. In the discussion of central tendencies in the following section, they are combined into a "median matrix," and later on they are split into time segments to show trends from the beginning to the end of the sessions.

FINDINGS

Central Tendencies in Exchange of Support. Taking the sample of forty-eight sessions as a whole, what evidence is there to confirm or to refute Simmel's observation that the elementary differentiating tendency is for the threesome to segregate into a pair and a third party? In order to represent the central tendencies in a simple manner, a median matrix is constructed as follows: rates for all cases in any one cell of the matrix (for example, the cell from the most active to the medium active) are taken to form a single distribution, rates in another cell another distribution, so that for exchange and total rates there are a total of twelve distributions, each with forty-eight cases. The medians of these distributions are recorded in a single matrix, shown in Table 2.

TABLE 2. MEDIAN RATES OF SUPPORT ACCORDING TO RANK ORDER OF CONTRIBUTIONS OF MEMBERS (N=48 cases)

As Initiator	As Recipient			Rate of Total Support Output
	Most Active	Medium Active	Least Active	
Most Active	12.0	7.0	9.7
Medium Active	11.1	3.8	8.9
Least Active	4.0	2.5	3.5
Rate of Total Support Intake	15.2	15.0	12.4

The highest rates of support are those exchanged between the two more active members and the rates are very near the same. Moreover, the frequency distributions, represented here only by the medians, are almost identical in every respect. All other distributions of rates are significantly different from these two. This is to say that as far as exchange of support is concerned, the relationship between these two members is sharply differentiated from the other relationships. The results for this sample confirm Simmel's observation. The two more active members form the pair and the least active member is the relatively isolated third party.

Interdependence of Relationships. The question of interdependence of relationships

is examined by holding constant deviations from respective medians in the most active pair and testing the association with deviations in the other two relationships. The classification of support rates above or below respective medians renders four types of relationships in the most active pair which are called: (1) *solidary*, when both rates are above; (2) *conflicting*, when both rates are below; (3) *dominant*, when the more active is below and the lesser active above; and (4) *contending*, when the former is above and the latter below.

Results of a standard chi-square test for association show that: (1) In the 15 sessions where the most active pair is *solidary*, there is association: the least active member opposes both members of the pair, and the medium active member reciprocates the opposition (chi-square values are 4.44, which is significant beyond the .05 level). Reciprocation from the most active member is not so regular (chi-square value is 2.28, significant at about the .16 level). (2) In the 13 sessions where the strongest pair is *conflicting*, there is no association whatsoever. The same is true for the 11 sessions where the relationship is *dominant*. (3) In the 9 sessions where the relationship is *contending*, the least active tends to support both other members (chi-squares are 3.56, significant at about the .07 level), but there is no association in rates of support given to him by others.

A comparison of average rates of support in respect to the least active member shows that they are lowest in the solidary pattern (intake: 6.57; output: 0.97) and highest in the contending pattern (16.10 and 6.84), with these latter rates not significantly different from those in the conflict pattern (15.17 and 5.37).

In summary, relationships are both differentiated and interdependent when the general tendency for segregation into a pair and an other becomes accentuated, the solidary pattern being the case in point. Here the least active tends to oppose the pair and the pair tends to reciprocate the opposition. Support to and from the least active is lower in this pattern than in others. However, when the tendency for segregation is reversed, as in the conflict pattern, there is neither differentiation nor interdependence. The least active member may not regularly depend

upon support from either of the others and apparently no regular pressures figure in his support of them. His position is indeterminate. With certain exceptions this is the case in the remaining patterns as well.

These results probably mean that Simmel's proposition of initial segregation is more relevant to the analysis of small group structures than his famous exemplification of *tertius gaudens*, for while the third member fares better when facing a conflict than when facing a solidary bond, he fares no better facing the conflict than he does in other patterns. The important interdependent pattern consists of the solidary pair in reciprocal opposition to the third member.

Tests for the Existence of a Power Structure. Before a pattern of relationships between members may legitimately be called a power structure it is suggested that it possess at least the following characteristics: (1) *internal differentiation in acceptance*; some members must receive more support than others, (2) *interdependence*; the differentiated elements must be related so that a change in one relationship will result in changes in others, (3) *stability of position* of members within the pattern through time, and (4) *stability of the pattern* itself through time.

Internal Differentiation and Interdetermination. Tests reported in the previous paragraphs show that differentiation is sharpest in the solidary pattern and weakest in the conflicting; that interdetermination is strongest in the solidary, next strongest in the contending, and non-existent in the conflicting and dominant patterns.

Stability of Activity Position. To test the constancy of activity position, the rank order of output of contributions during the first third of the session is compared with the rank order for the final third. Comparisons are made separately for each type of pattern. The probability of remaining in the same rank is tested by the following model:⁵ given NS as the number of non-shifts in rank order, R as the number of ranks (in this case,

⁵ The model is derived from the one used for the matching problem by Robert R. Bush and C. Frederick Mosteller, reported in the chapter, "Selected Quantitative Techniques," in *Handbook of Social Psychology*, Gardner Lindzey, editor (to be published by the Addison-Wesley Press, Inc., Cambridge, Mass.).

three), and n as the total number of members in all groups of a given type of pattern, then p is $1/R$; q is $R-1/R$; np is the mean number of non-shifts in rank order; and, the variance of non-shifts is $npq(n/n-1)$.

Results show that: (1) Activity positions are most stable in the solidary pattern; the probability of the observed number of non-shifts (34) is less than .01. (2) Activity position is least stable in the conflict pattern; the probability of the observed number of non-shifts (18) is greater than .05. (3) A large number of non-shifts for the most active members in the dominant and contending patterns render the probabilities of the observed number of non-shifts (20 in the dominant and 16 in the contending) less than .05.

In summary, all positions are stable beyond chance expectations in the solidary pattern, all positions unstable in the conflicting pattern, and, in the other types, while the position of the most active is relatively stable, positions of other members fluctuate.

Trends in Support Received. To test stability in this respect, gain or loss of support through time is calculated for positions in each of the four types of patterns.

It is found in the solidary pattern that though there is a considerable change in support rates from the first to the final third of the session, the changes amount to an accentuation of the pattern of support at the beginning of the session. The two or more active members gain in support (9.3 and 14.0 respectively), while the third member loses (-9.3). These trends, combined with the stability of activity position reported above, show that the "coalition" grows stronger through time as the position of the third member becomes weaker.

Again, trends in the conflict pattern are in contrast with the solidary. There is little gain or loss for any position (most active, -1.5; medium, -3.8 and least, 1.8). However, this stability is not only at a low rate but is largely a function of the rapid turnover in activity position, reported above. There is fluctuation in who takes part in the conflict, but the state of conflict remains fairly constant.

In the other two patterns, the most active member gains while the less active members lose; these gains and losses are more pronounced in the contending pattern.

Thus, in most general terms, it is found that fluctuation of activity position is related

to either a low rate or a loss of support, and that stability is associated with gain. The striking and important exception to this is the position of the third man in the solidary pattern; he both remains fixed in his position and loses support through time.

Stability of Patterns. Those patterns that maintain themselves throughout the session and toward which others shift are assumed to be stable, as distinguished from those that dissolve quickly and toward which no others shift.

Comparisons of patterns during the first phase with patterns in the third phase show that: (1) Of the 13 sessions with the solidary relationship, 10 remain solidary and three shift each to one of the other types of patterns. (Chi-square, in a test assuming equal likelihood of final pattern, is 15.9 which is significant beyond the .01 level). (2) Of the 19 sessions with the conflict pattern only 8 remain in conflict, while 5 shift to the solidary and 3 each to the other types. (3) Of the 7 with the dominant pattern, 5 shift to the conflicting pattern, and of the 9 sessions with the contending pattern, 4 shift to the conflicting, the others being distributed over the remaining types.

The single stable pattern is the solidary one; next in stability is the conflict pattern. The more transient patterns, the dominant and the contending, tend to shift to the conflicting. Temporally, the solidary and the conflicting patterns seem to be terminal tendencies.

Summary of Findings. Medians of support rates exchanged between three members in a series of 48 problem-solving sessions confirm Simmel's proposition that the primary tendency in the threesome is segregation into a *pair* and an *other*: the more active members form the solidary bond and the least active member is isolated. Tests for more detailed structural characteristics show that when this initial tendency is accentuated, there forms a genuine power structure, with internal differentiation, interdependence of relationships between members, stability of activity position, steady trends in receipt of support that are congruent with the initial differentiation, and, finally, stability of the pattern itself. This structure, in its ideal form of one positive relationship and two hostile relationships is called the "true coalition structure." Other patterns have some but not all characteristics

of a power structure—when the relationship between the stronger members is non-reciprocal, the pattern lacks temporal stability and when the relationship is conflicting there is rapid turnover in activity position and lack of interdetermination.

In so far as they represent divergent poles toward which groups may develop, the solidary and conflicting patterns seem the more interesting ones. In one direction power relations are structured with a steady maximization of the predictability of how members will act toward one another. In the other direction, there is maximization of uncertainty—uncertainty as to how one person's acts will affect another's, uncertainty as to who will lead and who will follow. In one direction there is differentiation, stability, predictability; in the other, lack of clear form, rapid fluctuation, unpredictability. The central relationship in one is positive, in the other, negative.

DISCUSSION

It has already been said that the findings confirm Simmel's basic point that the threesome tends to break up into a pair and another party. However, they do more than this; they enable us to add several points to his discussion. First, in his analysis of the dyad, Simmel was struck by the possibility of a sense of complete solidarity between the members, and, in progressing to the next larger size group, he assumed, in effect, the existence of this bond. Quite understandably, the added member became important primarily as an intruder or the disturber of this solidarity. It is possible but hardly necessary that fear of the third party accounts for the coalition formation, but, in any case, it seems plausible that once formed the coalition's intensity increases simply because there is a common object of opposition for each member of it. The third party, as scape goat, as common enemy, or whatever he might be called, may serve to cement the coalition as much as he threatens it.

The second point is in reference to the case of *tertius gaudens* which Simmel presents as a particular set of circumstances where conflict between two members results in benefit to the third.⁶ From the above observations, it is found that though this may have occurred

in one or two groups it is not a general tendency. However, the principle does seem involved in the case of the genuine power structure, the true coalition structure. As just stated, there is apparently a secondary gain for the coalition when one member of it opposes the third party. But note that this gain is directly dependent upon the existence of the coalition, for without it, the pattern develops into conflict and fluctuation of positions associated with conflict. This suggests that the principle of *tertius gaudens* can better be seen as an important dynamic aspect of the true coalition structure than as a principle underlying a type of structure all its own.

In view of these points, two simple propositions are suggested as additions to Simmel's principle of initial segregation: (1) The development most threatening to the position of any member in the threesome is the solidarity between the other two. (2) The condition most conducive to the intensification of a solidary bond, once formed, is the presence of a common object of opposition.

Another major point of Simmel's may be paraphrased to the effect that the three-person group is inherently unstable.⁷ If by unstable, he meant absence of a power structure, the findings are a clear contradiction of his position. If, on the other hand, he meant that in spite of a structure there are always residual strains, the question remains quite open. There is probably a basic incompatibility of interests and gratifications in the threesome, but to contend from this that residual tensions within members will result in a change in group structure is to assume a less rigid structure than shown by the data. Specification of the conditions under which residual tensions within members *do* result in a change in group structure is an important problem for further research.

Before they are of general use these propositions need further test. In fact, a rather long road of research seems required in order to determine to just what extent the above findings may be generalized either to other laboratory groups or, more important, to groups in less artificial settings, such as the family, the mediation situation, the tri-partite board, the play group, the therapy group, and the like. Not all groups of three are like the ones in this sample, and not all groups of

⁶ Kurt H. Wolff, *op. cit.*, pp. 154-162.

⁷ *Ibid.*, pp. 135-136.

similar composition operate under the same sets of conditions. Meaningful generalizations can be made only after the significant conditions are taken into account. No attempt is made here to carry out this task, even if it could be done with the present knowledge of groups. Instead, the writer should like to list some of the conditions surrounding the groups in this sample and conclude by posing several questions that should be answered in extrapolating to other three-person situations.

To mention some of the more important and obvious conditions: (1) the groups were *short-lived*—none lasted longer than two hours; (2) they were of *unusual composition*—all subjects were adult males, within a narrow age range, from a single and immediate academic community and all in need of part-time work; (3) the groups were of *unknown composition*—particularly in respect to personalities and admixtures of these in given groups; (4) the groups' *internal, functional organization was amorphous*; (5) they worked on *one task* and it was an unusual one; (6) while being observed they were kept *immune from most external influences and pressures*, other than those they brought with them; and, finally, (7) their *performance was divorced from reward and disapproval* from outside the group.

The list might be extended, but it is enough to say that unless other groups duplicate

these conditions any extension of the findings requires an investigation of the differences in accentuating or counteracting the tendencies observed for this sample. We must ask: (1) in spite of the differences in conditions, do the findings hold? (2) if not, what factors in the new setting account for the negative result? An example in respect to the present findings might be the family where coalitions are commonplace but intensive and continued rejection is rare. What factors not present in the laboratory groups account for this fact? For another example, it is clear that mediation collapses if coalitions *do* form. Is the hypothesis irrelevant in this case, or are there special, formal provisions that insure a check against "normal" tendencies in the three-party situation? Once these variables are specified, one may in turn ask a further question: (3) how may these variables that either intensify or negate originally observed tendencies be introduced into the group setting in the laboratory so that their effects may be measured?

It is in the light of these subsequent steps that the above findings are presented. They were not predicted, nor are they tests of a body of general theory. They are, however, relevant to Simmel's insights and it is hoped they will lend economy to the small groups researcher.

AN EXPERIMENTAL APPROACH TO THE STUDY OF STATUS RELATIONS IN INFORMAL GROUPS *

O. J. HARVEY

University of Oklahoma

THE present study is aimed at developing precise experimental indices of the status relations among members of small in-groups existing in actual life. This problem is approached through an investigation of the relationship between level of aspi-

ration on a given task and relative standing in the hierarchy of adolescent cliques. This is not, however, a study in level of aspiration *per se*. It is an application to the group level of the method of studying motivational and attitudinal factors through judgmental and perceptual processes.

Empirical studies of such groups by soci-

* This study was conducted as one unit of a coordinated research program on group relations at the University of Oklahoma under the direction of Dr. Muzafer Sherif. It was carried out under a contract between the Office of Naval Research and

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ologists¹ suggest the relationship we should expect to find between group status and aspiration level. Following the suggestions of such sociological works, Sherif generalized "that the individual's standards and aspirations are regulated in relation to the reference group to which he relates himself," and once an individual has gained status in this group, "his status aspirations and standards of attainment are determined accordingly."²

The methodological rationale of this study stems from a mounting number of experiments which show that even the most complex motives and attitudes of the individual reflect themselves in simple perceptual and judgmental reactions. It should be feasible, therefore, to devise techniques which will enable us to study status and role relations through variations in judgment or perception in laboratory situations.³

As has been shown,⁴ level of aspiration is determined by the *frame of reference* within which it takes place. The concept *frame of reference* as it is here used signifies the totality of external and internal factors that operate in a mutually interdependent way *at the given time*. Within the total reference frame some factor(s), internal or external, stand out and serve as anchorages for the total experience, the "outstanding kernel of the whole experience," to use Koffka's words.⁵ Applied to the immediate problem, this sug-

gests that although other factors may be causally related, one's group status, cloaked in definitive epithets and labels, may serve as the anchorage or salient variable in determining one's level of aspiration on a given task. One's expectation of his own and other group members' performance should be closely related to the expectations which have been defined in the group as appropriate to that particular status. The stronger the solidarity of the group, and thus the motivation of individual members, the more we would expect this to obtain—the extent to which it actually does offering an index of group cohesiveness and solidarity.

For the present problem this should mean that in cliques from slum areas of a large city one's expectations of his own and other clique members' future performance should be more closely related to the respective status positions occupied by each member than in groups coming from a background in which fewer factors operate to cause group solidarity. This greater relationship, if any, should be reflected in a higher correlation between status and aspiration level.

This position is lent concrete support by the findings of Whyte in his study of the "Nortons,"⁶ a gang in a slum area of a large eastern city of the United States. He relates that one time the "Nortons" became seriously interested in bowling, with performance in bowling coming to represent a mark of distinction in the group. As a consequence, a high performance by top-ranking members was accepted as "natural," but a high performance by members of low status was not tolerated because this did not conform to expectations. Low ranking members were put "in their place."

From the background provided by these and similar empirical findings our main general hypothesis was formulated as follows:

The level of aspiration of a member of an adolescent clique and the estimation of that member's future performance by other group members on a *task of significance to the group* bears a positive relationship to the relative position in the group hierarchy occupied by the individual whose future performance is being judged.

In order to investigate in a more precise way the differential expectations related to

¹ F. M. Thrasher, *The Gang*, Chicago: University of Chicago Press, 1927; W. F. Whyte, *Street Corner Society*, Chicago: University of Chicago Press, 1943.

² M. Sherif, *An Outline of Social Psychology*, New York: Harper, 1948, p. 124; p. 300.

³ M. Sherif, "A Preliminary Experimental Study of Intergroup Relations," in J. Rohrer and M. Sherif, editors, *Social Psychology at the Crossroads*, New York: Harper, 1951, especially pp. 421-423.

⁴ M. Sherif, *Psychology of Social Norms*, New York: Harper, 1936; D. W. Chapman and J. Volkman, "A Social Determinant of the Level of Aspiration," *Journal of Abnormal and Social Psychology*, 34 (1939), pp. 225-238; K. Lewin, L. Dembo, L. Festinger, and P. S. Sears, "Level of Aspiration," in J. McV. Hunt, editor, *Personality and the Behavior Disorders*, Vol. 1, New York: Ronald Press, 1946; O. J. Harvey and M. Sherif, "Level of Aspiration as a Special Case of Judgmental Activity in Which Ego-Involvements Operate as Factors," *Sociometry*, 14 (1951), pp. 531-583.

⁵ K. Koffka, "Perception: An Introduction to the Gestalt Theorie," *Psychological Bulletin*, 19 (1922), pp. 531-583.

⁶ Whyte, *op. cit.*

different status positions, this general hypothesis was subdivided into four specific hypotheses:

- (1) The higher one's status in the group the more he will tend to overestimate his own future performance on a given task.
- (2) The higher the group status of a given individual the greater will be the tendency of other group members toward overestimating his future performance.
- (3) The lower one's status in the group the less he will tend to overestimate his own future performance on a given task.
- (4) The lower the group status of a given individual the less will other group members tend to overestimate his future performance, even to the point of underestimation.

To examine the extent to which widely differing socio-economic backgrounds tended to accompany differences in group solidarity, a fifth specific hypothesis was added:

- (5) The correlation between extent of overestimation of future performance on a given task and status in the group will be higher for adolescents from a slum area than for adolescents representing a higher socio-economic background.

The hypotheses are to be tested in terms of the extent of overestimation of future performance on a significant task because this index has been found to tap personal involvements. It should be pointed out, however, that the extent of *overestimation* does not exclude the possibilities of *underestimation* of performance.

The task to be used has also been found to be meaningful and significant for the subjects to be studied,⁷ a *necessary prerequisite for any task purporting to tap personal involvements through level of aspiration investigations*.

The hypotheses gain further plausibility from the findings of Chapman and Volkman,⁸ Preston and Bayton,⁹ MacIntosh,¹⁰

Festinger,¹¹ and Gilinski,¹² which show the tendency for the level of aspiration to be shifted up or down by the alleged performance of members of groups considered inferior or superior to those of the individuals being tested. As the purported performance of a member of a specified group standing in an "inferior" or "superior" relationship to one's own group may serve as an anchorage causing increase or decrease respectively in level of aspiration, so we should expect that one's "inferior" or "superior" standing *within* a group should serve as an anchorage determining either lower or higher judgments of future performance on a given task.

SUBJECTS

Since its advent, the sociometric technique¹³ has been employed by many to ferret out the interpersonal relationships among group members. Some investigators have combined this device with teachers' ratings,¹⁴ and others have relied upon personal observations over an extended period in determining group structure.¹⁵

To insure well demarcated cliques and the selection of members who represented clearly defined status positions within such cliques, the present study employed all these devices in combination with the experimental technique. It incorporated objective observations (of teachers, counselors, and experimenter) and subjective evaluations (the subjects' own responses to a sociometric questionnaire) and experimentation with the aim of establishing an abbreviated experimental technique of detecting and predicting group status and role relationships.

The first step in selecting subjects for the final step of experimentation involved finding adults who had sufficient familiarity with

¹¹ L. Festinger, "Wish, Expectation and Group Standards as Factors Influencing Level of Aspiration," *Journal of Abnormal and Social Psychology*, 37 (1942), pp. 184-200.

¹² A. S. Gilinski, "Relating Self-estimate and the Level of Aspiration," *Journal of Experimental Psychology*, 38 (1949), pp. 256-259.

¹³ J. L. Moreno, "Who Shall Survive?" *Nervous and Mental Disease Monograph Series* (1934), No. 58.

¹⁴ M. E. Bonney, "Sociometric Study of Agreement Between Teachers' Judgments and Student Choices," *Sociometry*, 10 (1947), pp. 133-146.

¹⁵ Thrasher, *op. cit.*; Whyte, *op. cit.*

⁷ Harvey and Sherif, *op. cit.*

⁸ Chapman and Volkman, *op. cit.*

⁹ M. G. Preston and J. A. Bayton, "Differential Effect of a Social Variable Upon Three Levels of Aspiration," *Journal of Experimental Psychology*, 29 (1941), pp. 351-369.

¹⁰ A. MacIntosh, "Differential Effect of the Status of the Competing Group Upon the Level of Aspiration," *American Journal of Psychology*, 55 (1942), pp. 546-554.

some well-defined clique to rate its members in terms of their status in the clique. It has been shown that authority in the group affords a better indication of status than popularity *per se*.¹⁶ Consequently, those doing the rating were instructed to rate the members' relative standing on the basis of the authority they seemed to wield and the amount of activity they initiated for the group. However, it cannot be said with perfect assurance that those doing the rating always followed these criteria in making their judgments.

Subjects were selected from two major socio-economic groups, which *in relation to each other* may be termed higher and lower. Subjects of the higher group (H. S. Group) were of middle class professional parents most of whom were academically attached to the University of Oklahoma. The subjects of the lower group (L. S. Group) were of laboring parents, some unemployed, who lived in a slum area of Oklahoma City. The neighborhood in which these subjects lived was an interethnic one containing a large number of Mexican families, a few Negro families, and some Anglo-American ones.

All the cliques in the H. S. Group were independently rated by two of their teachers who had known them for at least a year. Since these subjects were attending the University School which contained very small classes (12 to 15 students per grade) the teachers were in an excellent position to know the relative standing of each member in the clique hierarchy.

In addition to teachers' ratings, the members of one of the cliques in the H. S. Group were rated by the experimenter on the basis of his observations of it over a period of two months. This clique, comprised of five eighth grade boys, was observed in a situation that allowed for a rather wide freedom of interaction, namely, a small snack bar close to their school where these boys ate lunch every day. Although almost the entire population of the University School lunched in the school cafeteria, these five boys chose to remove themselves and eat away from the other students.

To gain as clear a picture as possible of authority in the group, special attention was paid to such indications as who chose the table at which to sit (if the customary one

was taken), who had his order brought to him and by whom it was brought, to whom conversation seemed to be most often directed, who told the "funniest jokes," who seemed to initiate departure, and around whom the boys centered when they started walking back toward school. No notes were made in the presence of the group but immediately after its departure the experimenter recorded his observations.

All the cliques in Group L. S. were attending a large public junior high school, the students of which represented very similar socio-economic backgrounds. Since this situation provided less opportunity than in the case of the H. S. Group for each teacher to be intimately familiar with each clique, all clique members of the L. S. Group were independently rated by *five* teachers who were sufficiently familiar with the groups to make such ratings. In addition, some of these cliques were rated by the recreational director and coach at the Salvation Army Center, with which they had been affiliated for as long as two years.

Cliques thus selected and rated, by persons in key vantage positions, as being characterized by clearly delineated status relations were then administered a sociometric questionnaire which was further aimed at discovering positions of authority in the group rather than simply popularity. On the questionnaire, subjects were asked to indicate who most often and least often thought up things for them to do, who would be most likely and least likely to select the movie all would want to see in case there was indecision, and who would be elected president and who would receive no office if the group organized a club. Questions as to whom one would prefer to sit by in school, to have as tentmates on a camping trip, as well as questions intended to reveal whether or not subjects maintained their membership in the clique in and out of school were also asked.

To check the awareness of the existing group structure, subjects were asked, *at the end of the questionnaire to prevent biasing preceding answers*, whether or not any member in the clique could be called the leader, and if so, who. No limit was set on the number of choices a subject could make nor the boundaries set to define who should be included in the choices. The subjects were all

¹⁶ Whyte, *op. cit.*

instructed that the reason the questions were being asked was to find out with whom they would like to throw darts so that, in addition to being paid 50 cents an hour, they could enjoy the game even more. To offer a greater freedom of choice, subjects were assured that no one but the experimenter would see the results.

The sociometric questionnaires were scored by a system of weighting in which first choices were allotted a weight of five, second choices four, third choices three, fourth choices two, and all below fourth choices one. This was done for each of the six significant questions. Weighted choices were then summed and the total score of a subject was taken to represent his relative standing in the group. The procedure of weighting choices was followed to allow for the different intensities in relationship represented, for example, between a first and fifth choice.

The results obtained by the sociometric questionnaire were then compared with the adults' ratings. From groups where there was perfect agreement between these two indices, and in the case of the group observed by the experimenter, the experimenter's ratings, on the leadership and lowest positions, three subjects were selected: the leader, the lowest ranking individual, and a third individual whom all ratings had placed somewhere near the middle of these two extremes. The adherence to the strict criterion of agreement between teachers' ratings and the subjects' own evaluations meant the elimination of six cliques which had satisfied all the requirements up to this point.

The three subjects thus selected, from each clique usually consisting of from four to six individuals, then participated in the main experiment in which each one estimated his own future performance and that of the other two members on a task desirable to the subjects.

Ten groups of subjects, each containing the leader, middle ranking and lowest standing members of an adolescent clique, were used in the final experimentation. These 10 cliques were selected from among 16 such groups which contained a total membership of 85 individuals. The six groups of 35 individuals which were not subjected to the final experimentation were eliminated because of failure to meet the criterion of agreement

between the sociometric results and teachers' (counselor, experimenter, coach) ratings.

Four of the cliques (six males and six female subjects) which participated in the final experimental situation were of the H. S. Group. These subjects were all tested in a laboratory on the North Campus of the University of Oklahoma, one block from the University School which they attended.

The remaining six experimental cliques (12 male and six female subjects) were of the L. S. Group. All of these were tested at Oklahoma City in an experimental room provided at the Salvation Army Center in the neighborhood where many of the subjects came for outside recreation.

APPARATUS

The apparatus was the same as that reported earlier by Harvey and Sherif.¹⁷ It consisted of a specially constructed dart board and five feathered darts. The dart board included a detachable frame on which was stretched a target containing 10 concentric circles ranging in numerical score value in steps of two from the lowest value of 2 in the outside circle, to the highest value of 20, the inside circle or bull's eye. Behind this target, mounted on beaverboard, was another target of the same size but without the concentric circles. The first target was displayed only to allow the subjects to familiarize themselves with approximate score values of given areas on the target. The psychological rationale for this has been presented elsewhere.¹⁸

PROCEDURE

The procedure is also the same as that previously followed.¹⁹ Subjects were instructed that the experiment was a hand and eye co-ordination test, an explanation formerly found to be appropriate and acceptable to the subjects. To aid in keeping the subjects doing their best, they were reminded that, although they would not be told their scores, 20 was the best possible score and zero the worst. In the earlier study it was found that although subjects were not told their score they were able to estimate with very high

¹⁷ Harvey and Sherif, *op. cit.*

¹⁸ *Ibid.*

¹⁹ *Ibid.*

accuracy the score actually made on each trial.

The subjects were cautioned always to estimate the score they *actually expected to make* or be made, not the score they *hoped to make* or be made. Each subject was, after 10 practice trials, given 50 trials—the throwing of one dart representing a trial. Before each trial the subject who was throwing called aloud and recorded the score he expected to make on that particular trial, and after each trial he called aloud and recorded the score he judged he actually made on the dart just thrown. The other two subjects only recorded their estimates of performance of the one who was throwing, before and after each trial. They made no judgment aloud. As soon as they had recorded their estimates they signified this to the one throwing at which time he called his judgment aloud.

The order in which the darts were thrown was always determined by the subjects themselves. The tendency was for the order to follow status, with the leader throwing first and the lowest ranking member throwing last. Usually the subjects consented unanimously to this order.

After each subject had thrown 50 darts a questionnaire was administered which was aimed primarily at ascertaining expectations each member held for himself and the other group members before they began to throw.

All subjects were tested only in the presence of the experimenter. All were paid 50 cents an hour for their participation.

RESULTS

To test the hypotheses it was necessary to determine the extent to which each subject overestimated or underestimated his own future performance and that of the other two status occupants, and then to correlate this with group status.

The first step was carried out by subtracting from each estimate of future performance the score judged to have been made (by oneself or the other members, depending on who was throwing) on that particular trial. The differences for the 50 trials were summed and divided by N, giving the mean differences between estimates of future performance and judgments of actual performance as it was perceived by the subjects. These

mean difference scores, following the usual practice, are referred to as *D-Scores*.

For each subject three D-Scores were computed, the difference between his own aspirations and judgments of actual performance, and the similar differences for his judgments of the performance of each of the other two members. D-Scores of a positive value would, of course, indicate *overestimation*, and negative D-Scores would show *underestimation*.

The first four hypotheses are to be tested by comparisons of D-Scores of each status representative for his *own* performance and the D-Scores of each member for his estimates of the performance of *other* higher or lower status members.

The results given in Table 1 are pertinent to hypotheses one and three. From this table presenting the combined data of all groups, it can readily be seen that the higher an individual's status in an adolescent clique

TABLE 1. MEANS OF D-SCORES OF THE LEADER, MIDDLE RANKING AND LOWEST STANDING MEMBER FOR THEIR OWN PERFORMANCE

Status Position	
Leader	3.16
Middle	2.49
Low	.90

the more he will tend to overestimate his future performance on a significant task.

The relationship between mean D-Scores for the three status positions was found to exist whether the results for the two groups, H. S. and L. S., were combined or analyzed separately.

While subjects in Group H. S. tended to overestimate their own future performance to a somewhat greater extent than did individuals in Group L. S., the differences were insignificant.

Although not shown in the table, it is important that three of the lowest ranking subjects actually *underestimated* their own future performance. In contrast, none of the top ranking or middle standing subjects underestimated their own performance.

The three individuals, two from Group L. S. and one from Group H. S., who actually performed at levels higher than they estimated appeared to be struggling to maintain their standing in their groups. It can be

TABLE 2. DIFFERENCES BETWEEN D-SCORES FOR ESTIMATES OF OWN PERFORMANCE BY THE LEADER, MIDDLE AND LOWEST STATUS MEMBERS

		Status Position Being Compared		
		Leader-Middle	Leader-Lowest	Middle-Lowest
Mean Diff.		1.15	2.75	1.56
t		1.966	9.407	2.130
P		<.10	<.001	<.10

assumed that as a consequence they conformed completely to the group expectations of its lowest ranking members. To remain in good standing these individuals were "more Royalist than the King himself."

Table 2 presents the difference between D-Scores for own performance by subjects of the three status positions.

The greatest difference, as should be expected, lies between the magnitude of the D-Scores of the leaders' estimates of their own performance and the D-Scores of the lowest ranking members' own performance. This difference is significant at below the .001 level. The differences between the D-Scores of the estimates of their own performance by the leader and middle standing individual, and the difference between the D-Scores of the middle and lowest ranking individuals for their own performance were both significant at below .10 level of confidence.

From these data we can deduce that the greater the relative distance between status positions the greater are the differences in the expectations occupants of these positions hold of themselves.

The *higher* a member's standing in the group hierarchy, the *more* he will tend to overestimate his future performance on a meaningful task. Conversely, the *lower* an individual's relative standing in the group structure, the *less* will he tend to overestimate his future performance, even to the point of actual underestimation in some in-

stances. This represents a confirmation of hypotheses one and three.

A general statement of our second and fourth hypotheses is that the extent to which other group members tend to overrate the performance of a given individual depends upon the relative standing in the group hierarchy of that particular individual. Relating to these hypotheses are the results presented in Table 3 which shows the mean D-Scores of the leader, middle standing, and lowest ranking, members based on their estimates of the performance of *other* group members occupying higher or lower status positions. Table 1, it will be recalled, gave the mean D-Scores based upon the estimates of his *own* performance by each of the status representatives.

The data for Group H. S. and Group L. S. were also analyzed separately. The trend revealed for each group when considered separately was the same as that in the combined results presented in Table 3.

These results permit the inference that the *higher* the standing in the group hierarchy of a given individual, the *greater* are the expectations other group members hold of him. Thus the *higher* a subject's status the *more* his future performance was overestimated by lower ranking subjects. Conversely, the *lower* the standing in the group hierarchy of a given individual the *lower* were the expectations other group members tended to have for him, so much lower in fact that it was the tendency for higher standing members to *underestimate* the performance of the lowest standing member. Thus hypotheses two and four are substantiated.

Supporting this generalization in a summary but crucial way are the results presented in Table 4 which were obtained by having each subject indicate on a five-point scale ranging from very well to very poorly the quality of performance he had expected of each of the three status representatives, including himself, before the darts were thrown. Greater accentuation of these re-

TABLE 3. THE MEAN D-SCORES OF THE LEADER, MIDDLE AND LOWEST STATUS MEMBERS BASED ON THEIR ESTIMATES OF THE PERFORMANCE OF OTHER GROUP MEMBERS OCCUPYING HIGHER OR LOWER STATUS POSITIONS

Status Position Being Rated	Leader		Middle		Lowest	
	Middle	Lowest	Leader	Lowest	Leader	Middle
Status of member judging	2.99	2.45	1.66	.98	-.13	-.18

TABLE 4. THE PERCENTAGE OF SUBJECTS EXPRESSING GIVEN LEVELS OF EXPECTANCY FOR THE PERFORMANCE OF THE LEADER, MIDDLE AND LOW STANDING MEMBERS

Status	Quality of Expected Performance				
	Very Well	Pretty Well	Had No Idea	Quite Poorly	Very Poorly
Leader	13.3	73.3	10	3.3
Middle	46.7	46.7	6.6
Lowest	36.7	43.3	20

sults may have occurred had the ratings been made *before* the darts were thrown, before actual performance of the respective members had a chance to exert its full weight. However, such accentuation was forfeited as an additional precaution against the subjects' suspecting the real purpose of the experiment.

From this table it can be deduced that it was the leader for whom subjects most frequently held high expectations, and it was the lowest standing member who most members expected to perform poorly.

The chi-square test revealed that the frequency of individuals indicating they had expected the leader to do very well or pretty well was significantly greater than the number expecting either the lowest ranking or middle standing subject to do well (P below .001 in both cases). However, the difference between the number expecting the middle standing member to do well and the number expecting the lowest standing individual to do well was not significant.

To determine whether or not a member of a given status tended to overrate more his own performance than the performance of members above or below him in relative group standing, the D-Score of each individual for his *own* performance was compared with the D-Score obtained from the judgments by this *same* individual of the performance of each of the other two status

occupants. These comparisons consisted of subtracting the mean value contained in Table 1 from corresponding values in Table 3. The results obtained from these comparisons are presented in Table 5.

From this table certain inferences meaningful to any theory of self-perception can be drawn. The higher one's status the *more* he will tend to rate his performance above that of individuals with status lower than his own.

Conversely, the *lower* one's standing in the clique hierarchy, the more he will tend to rate his own future performance *lower* than he rates that of members occupying a status above him.

While the findings for each of the Groups H. S. and L. S. are in agreement with the results from the combined sample, certain differences between the two sub-groups that may be taken as a reflection of differences in group solidarity should be pointed out.

An index of solidarity, as we have suggested, is the relationship between status occupied and magnitude of the D-Scores for the occupant of the particular status position. If there should be perfect correlation between these two variables, every higher ranking member would have a greater D-Score for his own performance than for the performance of members below him in status. Each lower ranking member would have a smaller D-Score for the estimates of his own per-

TABLE 5. DIFFERENCES BETWEEN D-SCORES FOR *Own* PERFORMANCE AND D-SCORES OF ESTIMATES BY THE *Same Individual* FOR THE PERFORMANCE OF GROUP MEMBERS OCCUPYING HIGHER OR LOWER STATUS POSITIONS *

	Leader-Middle	Leader-Lowest	Middle-Leader	Middle-Lowest	Lowest-Leader	Lowest-Middle
Mean Diff.	1.98	3.77	-.50	2.67	-1.55	-.08
t	3.661	6.350	-1.173	4.182	-3.165	-.145
P	<.01	<.001	.30	<.01	<.02	.90

* In each column heading the status mentioned first is that of the member who is estimating his own performance and the performance of the subject whose status is mentioned second. For example, Leader-Middle column gives the difference between the leader's estimate of his own performance and the leader's estimate of the performance of the middle status member.

formance than for his estimates of the performance of members above him in status. In such a case, it could be said that group members had come to accept their relative positions in the group hierarchy and, as a consequence, the particular group was characterized by a very high degree of solidarity.

It does not appear as important that higher ranking group members should rate their own performance *above* lower ranking members as that lower standing members would actually place their estimates of their *own* performance *below* their estimates of the performance of higher ranking members. In a society such as ours, it is the usual thing for individuals to try, and expect, to surpass the performance of friendly competitors. Therefore, the extent to which group members actually rated their *own* performance *below* that of higher status occupants could be taken as a significant indication of the degree to which members had seemed to accept their status in the group hierarchy and, as such, would afford a valuable index of group solidarity.

In Group L. S. the tendency was more pronounced than in Group H. S. for lower standing individuals to rate their own performance below their estimates of the performance of higher ranking members. In Group L. S. the D-Scores of the lowest ranking members for their own performance were, on the average, more than three times farther *below* the D-Scores of their estimates for the performance of the leader than was the case in Group H. S. The mean difference in D-Scores based on the estimates of their own performance by the lowest ranking members and D-Scores of the estimates by these same individuals of the performance of the leaders were -2.15 for Group L. S. and $-.65$ for Group H. S.

Furthermore, in Group L. S. the mean difference in D-Scores of middle ranking subjects for their own performance and for their estimates of the performance of the leaders ($-.97$) was greater than the comparable difference ($-.04$) for the middle ranking subjects in Group H. S.

Similarly, lowest standing subjects in Group L. S. more often rated their own performance below that of middle status individuals than did comparable individuals in Group H. S. These mean differences for

Group L. S. and Group H. S. are $-.22$ and $.03$, respectively.

From these results it would appear that subjects in Group L. S. had come to accept their relative standings in the clique hierarchy to a greater extent than had individuals in Group H. S. and, as a consequence, greater cohesiveness existed among the cliques from the slum area.

This position gains added plausibility from the findings related to hypothesis five in which it was proposed that a higher correlation should exist between magnitude and direction of D-Scores and group status for cliques from Group L. S. than for those from Group H. S. To test this, it was necessary to rank each status representative of each group in terms of magnitude of D-Scores of all members for his performance.

Since the performance of each individual was estimated by three persons there were three D-Scores relating to the performance of each subject. To get just a single rank order value with which to correlate status, each of the three D-Scores for each status occupant was ranked in magnitude as 1, the highest, 2, intermediary, and 3, lowest magnitude. The values of these rank order positions were then summed and divided by 3. It was this single rank order value that was then correlated with status position.

The correlation analysis was carried out by use of the gross score formula. Rank order correlation was inapplicable since there were ties in rank positions. The resulting coefficient is $.83$. The t value is 39.8 , which indicates significance beyond the $.001$ level.

From these results it can be inferred that a significantly high positive relationship obtains between status in well defined adolescent cliques and estimation of performance (as indicated by D-Scores). Not only are the expectations that group members have of a given individual related to the status of the member being judged and the one estimating, but also, and *more significant for ego-psychology, the level at which one sets his expectations of his own performance on a task seems to be very highly related to his standing in the group.*

High status in an adolescent clique tends to be accompanied by generally high expectations by all members for the occupant of the high status position. Conversely, low expectations of performance tend to be held

of low ranking members by all members of the group. Thus our general hypothesis is substantiated.

The fifth hypothesis was tested by comparing the correlation value obtained for Group H. S. with that derived for Group L. S. Table 6 presents the results of this comparison.

For subjects from both the slum area and professional homes a significant correlation was found to exist between judgments of performance on a task and status in the

TABLE 6. CORRELATION BETWEEN D-SCORES AND STATUS IN ADOLESCENT CLIQUES FOR GROUPS H. S. AND L.S.

Socio-Economic Group	r	t	P
H. S. (N=12)	.783	5.360	<.001
L. S. (N=18)	.856	6.636	<.001

adolescent clique. The difference of .07 between the correlation values is not significant. However, since it is in line with results discussed earlier, this difference is further suggestive that greater solidarity existed among the cliques from the slum area than from professional homes. The extent to which the difference found between the two groups represents reflections of consistent differences in solidarity remains to be answered by future research, preferably with larger samples.

DISCUSSION

The status relationships and reciprocal expectations revealed behaviorally in these results need not be consciously expressed by the subjects themselves. This fact was revealed by the responses of the subjects to the questionnaire administered after the experimental session. In the 10 experimental cliques only eight subjects (less than one-third) indicated that they thought some particular person in the group could be called the leader. Several subjects expressed this idea in some such words as "We just go by the majority."

As Whyte has pointed out,²⁰ the structure of the group need not be explicitly recognized by the members for it to exert its in-

fluence on the psychological activity of the members. This is inferred on the basis of the differential reaction of members toward each other and toward outsiders.

Among the most significant findings of this study for ego-psychology, and especially for self-perception, is the high correlation found between what one expects of oneself and what other group members expect of him. The greater the significance of status in a group, and group solidarity, the more we would expect this relationship to hold true.

This relationship should be greater for cliques whose members are welded even more tightly by inadequate satisfactions in the home and other groups outside their clique, and by strong pressures against the clique from outside sources such as are directed against delinquent gangs.

As a probable reflection of this, a higher correlation was found, between status and expectations of oneself and other clique members, among boys coming from a slum area of a large city than for subjects from more adequate professional homes. This, coupled with findings discussed earlier, seems to indicate that on the whole the cliques from the more underprivileged background were characterized by greater solidarity than those from professional backgrounds in which circumstances conducive to such strong clique ties are expected to operate to a lesser extent.

Our findings seem to point to the conclusion that the expectations an occupant of a given status in a *well-defined* informal group holds of himself are largely determined by the expectations which have become defined by the group as appropriate to that status. As any group value or norm which is internalized may serve as an anchorage in determining judgments and perceptions of related situations, so it seems that the definitive labels and epithets attached to each status position, and thus to its occupant, by the group may serve as salient anchorages in determining one's judgments and perception of one's self as well as the other group members. Our knowledge of intergroup relations, typified by such a phenomenon as prejudice, shows our perception of and reaction to members of out-groups to follow this same general principle, that is, to take place on the basis of norms of the group which pre-

²⁰ Whyte, *op. cit.*

scribe the relative distance out-group members are to be held from in-group members, and thus the appropriate behavior toward the out-group members.

Certainly, an outstanding problem for the whole area of ego-psychology is to determine to what extent our self-perceptions or conceptions, our very self-esteem, are a function of our group status and the definitive labels assigned it—in the family, play groups, adolescent cliques and adult reference groups. Such work would contribute immeasurably to the understanding of how broad cultural and social processes operate to influence the individual. At the same time a more solid ground will be laid, on which to advance to the study of intergroup relations, by showing that one perceives and reacts to himself, other in-group members, and out-group members in line with the prescribed norms of his group.

The results obtained point to the feasibility of using simple judgmental processes in an experimental situation as indices of status and role relations existing in small group structures. As such, it can serve for validation of interpersonal relations obtained through the use of sociometric and other devices in this area.

Being *indirect*, in the sense of a projective technique, less time consuming, and lending itself to quantitative analysis of data, such a technique may prove to be an effective approach to the study of status and role relations.

The sensitivity and applicability of this indirect experimental method of discovering role relations through judgmental activity, of course, rests upon finding the gradation of structure of the experimental situation appropriate to the detection of experimentally introduced variables (social status, role relations and other motivational factors).

SUMMARY AND CONCLUSIONS

The present study aimed at establishing a short-cut experimental technique of determining the status and role relationships existing in small informal groups, and at the same time undertook to validate sociometric indices against an experimental situation.

The general hypothesis was that the level of aspiration of a member of an informal

clique and the estimation of that member's future performance by other group members on a task of *significance to the group* bears a positive relationship to his relative position in the group hierarchy. From this general hypothesis, five specific hypotheses were derived. Ten adolescent cliques singled out from among 16 such cliques on the basis of agreement between teachers' ratings, personal observations and sociometric results (subjects' own evaluations), were subjected to the experimental situation. Three members from each clique—the leader, middle ranking and lowest standing members whom all the criteria had placed at these respective positions, were given the task of estimating their own future performance and that of the other two status occupants on an experimental task.

Four of the experimental cliques consisted of individuals from professional families living in a university town. The other six experimental cliques came from families of unskilled laborers living in an inter-ethnic slum neighborhood of a large city.

From the results of the experiment the following conclusions were reached:

(1) The higher one's status in the group the more he tended to overestimate his own future performance on a given task.

(2) The higher the group status of a given individual the greater was the tendency of other group members to overrate his performance.

(3) The lower one's status in the group the less he tended to overestimate his own performance.

(4) The lower the group status of a particular individual the less other group members tended to overrate his performance, even to the extent of *underestimating* it.

(5) The correlation between extent of overestimation of performance on a given task and status in the group was higher for individuals from slum areas of a large city than for subjects from a higher socioeconomic background. This was taken as suggestive of greater solidarity among the cliques from the slum area.

The level at which one sets his expectations of his own performance on a task is thus highly related to his standing in the group.

SOME FUNCTIONAL CONSEQUENCES OF PRIMARY CONTROLS IN FORMAL WORK ORGANIZATIONS

EDWARD GROSS

University of Washington

THE present paper concerns the proposition that primary controls may be functional within instituted social organizations. For this purpose, evidence will be submitted which suggests that such primary controls may make their appearance precisely when formal or institutional controls prove inadequate. As such, these primary controls enable the organization to accomplish its formal purpose.

The study uses two sources of data—one, a manufacturing concern in Chicago, with approximately 1500 workers, which produces plastic raincoats, belts, jewelry and other items; and second, a radar airsite in the Air Defense Command, United States Air Force.¹ The two cases are utilized first in order partially to avoid the dangers of the unique case, and second, in order to provide a comparison between a small, independent civilian factory on the one hand, and a huge interlocking military organization on the other. The data in both cases were gathered by means of interview and participant observation techniques.² It is

necessary to emphasize that the writer is still very much in the midst of the Air Force study, so that findings with reference to it are highly tentative.

Work organizations are characterized by purposiveness. Each has one or more specified goals or objectives which it seeks to achieve. In the case of the factory, these are, mainly, the manufacture, sale, and distribution of certain physical goods, while in the case of the airsite, there is the one objective or "mission" of maintaining radar surveillance against hostile aircraft. The fact of purpose creates the need for institutional or segmented controls on behavior so that this purpose or mission may be realized. To this end, it is necessary only that a segment of the personality be controlled; namely, the role as worker, but it is essential that *that* one role be controlled. This control is provided in the following manner in the two organizations under examination.

Formal control is instituted through a division of labor which serves to relate specialisms and specialists to one another, through an authority system which serves to coordinate specialists and to evaluate their contributions to the objective, and finally, through a selection system which serves to recruit personnel who, it is felt, can assume the work roles required.

We proceed next to discuss each of these instituted controls and the manner in which they may break down or prove inadequate, and how, in each case, primary controls then make their appearance. The paramount prob-

did some work with the factory personnel. There were approximately 100 workers in the office at the time of the study, and a total of 319 interviews (nondirective) were gathered. It is difficult to specify the nature of the Air Force population because of security regulations. However, it may be said that the writer has been with the project on a full-time basis for one and a half years, of which approximately one half has been spent at air sites. The writer is concentrating the major part of his efforts at one air site.

¹ The writer is on the professional staff of Air Site Project, a research organization within the Department of Sociology at the University of Washington. The project is under the direction of D. C. Miller. The research reported here was supported in part by the United States Air Force under Contract Number AF 33(038)26823, monitored by the Human Resources Research Institute, Air University, Maxwell Air Force Base, Alabama. Permission is granted for reproduction, translation, publication and disposal in whole and in part by or for the U. S. Government.

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² For a full description of method in the factory study, see Edward Gross, "Informal Relations and the Social Organization of Work in an Industrial Office," unpublished Ph.D. Dissertation, University of Chicago, 1949, Chapter 1. The research the writer did there was concentrated in the office among the white-collar workers, though he also

lem which any division of labor system must solve, as stated above, is that of relating the specialisms and specialists it creates to one another. Observation revealed that the following instituted means were used to attempt to solve this problem: (1) Minute specialization, and (2) Provision for formal horizontal communication. In general, specialization became more minute as operations increased in complexity. Each predictable act was planned for in advance and assigned to one or more persons. These acts were related to one another in flow charts, with the underlying conception that, if each person did what was expected of him, then specialisms would mesh and the goal be reached. It was felt that the more minute the division of labor, the easier it is to train persons and replace them. Further, the more carefully specialisms were defined, it was felt, the less likely would it be that specialists would overlap and conflict with one another. Formal horizontal communication between specialists, in turn, was expected to take place through a common supervisor, so that appropriate permission was secured and relevant persons informed of the action.³

But in actual practice, at both the factory and the air site, these instituted procedures gave rise to problems. The provision for formal communication through a common supervisor was found to be cumbersome and time-consuming. The required person was often busy, or, at the air site, which is on 24-hour shift duty, he was off-shift, and thus action came to a standstill. Yet some persons were loath to take action by simply going directly to the specialist concerned, while others did so, but in interviews, expressed anxiety about possible reprimand.

The minuteness of the division of labor had two effects in both organizations.

(1) While *specialties* were related to one another, the very minuteness of the division of labor and the restriction of persons to their specialties operated effectively to isolate *specialists* from one another. Thus, when pressure was imposed for each person to do his job, persons sometimes refused to help others in difficulties. Most interesting was the expression in interviews of what might

be called a "sphere of concern." One office worker in the Credit Department at the factory stated:

"I check ledger cards to see what a man's past credit record with the Company is. All I want from the Accounting Department is clear, up-to-date postings on the cards. I don't know where they get their information and I don't care. I'm paid to do a job here, and how they do theirs is none of my affair."

On a later occasion, when mailing lists for the company catalogue were being drawn up, extensive checking of accounting records was necessary to determine who were to receive catalogues. The company experienced difficulty in getting persons from other departments in the office to assist in this emergency work, which required meeting a publication deadline. Persons stated that they were unfamiliar with accounting records, and, more important, could see no point in leaving their own work to assist the accounting personnel. The formal division of labor provided no way for rewarding such effort, unless a supervisor saw fit to recommend a raise or promotion, because such extra effort came to his personal attention. It is noteworthy that such a reward is only possible in an organization small enough for higher supervisors to be in close touch with lower employees.

At the radar site, a similar problem occurred in connection with specialty classification. Men are originally classified at induction depots by AFSC⁴ and given commensurate training. In order to justify the time and money spent on a man, it is required that he spend at least one year in his AFSC before he is eligible for a change. Further, promotions in rank for airmen depend to a considerable extent on time spent in the AFSC. The job of radar operator was reported in interviews to be a highly tedious one for which it is apparently difficult to recruit volunteers in large numbers; therefore, some men are given that AFSC even though their primary wishes lie elsewhere. Now, some commanding officers have adopted the procedure of shifting men temporarily to other specialties wherein there are personnel shortages, and where those specialties are more in accord with the men's interests and

³ See B. B. Gardner, *Human Relations in Industry*, Chicago: Richard D. Irwin, Inc., 1945, Chap. 2, for a discussion of this point.

⁴ An abbreviation for Air Force Specialty Code, a system of classifying specialties.

wishes. This informal practice might actually assist the air site in accomplishing its mission more effectively. However, the restriction on promotion interferes, and men discover they are not accumulating time in their official AFSC's. Thus, they find themselves caught in the midst of a dilemma for which the formal system provides no solution. One consequence is considerable anxiety, "griping," and requests for return to the official AFSC. Thus, as in the industrial office, specialization made it difficult for men to cross specialty lines when such action was necessary to the mission of the organization, and the promotions system is tied up inextricably with the specialization system.

(2) The restriction of persons to their specialisms had the further effect of tending to strip work of meaning. Indeed, it seemed that the greater the restriction, the more meaningless the work. This was found to be the case in the small civilian organization but it seemed to be much more the case in the Air Force organization. Radar surveillance is but a minuscule segment of Air Force activities, and, in spite of movies and the Information and Education program, men in interviews expressed difficulty in being able to see where their particular activity fitted into the whole. This led, in some cases, to a loss of faith in the mission, with consequent depression and feelings of uselessness. The phenomena of payday sprees and debauches may be, in part, a function of this problem.

However, further examination revealed that a second set of controls was operative, which lessened the severity of these problems and even prevented their occurrence in some cases. These controls were primary in nature, and took the form of the clique or informal group, wherein workers met one another as more than segmented personalities. In the case of the office, it was discovered that the 11 cliques that were found in the office tended to cut across work sections, and thus brought together, on an informal basis, persons from various segments of the structure. Within these groups, horizontal communication was easy, persons volunteered assistance to one another, and, by being able to compare their specialisms with one another, gained some conception of the relation of their work to the whole. At the

same time, isolates proved to be peculiarly vulnerable to the inadequacies of the institutional controls.⁵

In the case of the air force organization, a special situation prevailed by virtue of the fact of 24-hour-a-day shift operations. It is the practice to change men continually from one shift to another, so that, in a short period of time, in certain sections, a man has worked every shift. The writer, with two graduate student associates, L. W. Wager and H. J. Loether, became aware of what we have since called "work leisure cycles." The concept of work leisure refers to the amount of opportunity afforded by a specialty for informal recreational contacts while on the job. Some shifts coincide, but others run at different rates around the clock. Preliminary findings suggest that cliques tend to develop between work sections when their work leisure opportunities coincide. This in turn, makes possible inter-specialty informal communication.

By contrast, one work section operates a special piece of equipment, while another work section does this section's maintenance work for it. Their shifts coincide, but here we have a case where work leisure periods do not coincide. When the operators of the equipment are working, maintenance personnel merely stand by. When the equipment requires repairs, or is given periodic maintenance checks, operators have leisure, but it is just then that maintenance personnel are busiest. Observation has revealed an almost complete absence of cliquing between these two work sections. Instead, there is considerable conflict between them, refusals to make allowance for each other's problems, and actual lack of knowledge, in some cases, of how interdependent they in fact are and must be. Solution of these conflicts requires intervention by the commanding officer. These data, while only suggestive, imply that cliques which cross specialty lines act to reduce conflicts between work sections, by virtue of the intimacy provided by clique interaction. But if cliques are prevented from forming by a lack of coincidence of leisure periods, then conflict may, and

⁵ For a fuller discussion, see Edward Gross, "Characteristics of Cliques in Office Organizations," *Research Studies of the State College of Washington*, Vol. XIX (June, 1951), pp. 131-136.

does occur. The writer has not had the opportunity to observe this work leisure phenomenon in civilian industries, but it is noteworthy that cliques in the civilian office tended to be composed of persons who had coffee "breaks" at the same time of day, and who ate lunch at the same hour. The times for the latter were different for different departments.

We turn next to the formal authority system. A work organization requires some means for coordinating and evaluating the work done in order that it shall be consistent with the purposes of the organization. In organizations of any size, this is usually accomplished by establishing a set of supervisors of graded rank who are held responsible for segments of the organization.⁶ Institutional requirements in both organizations under examination provide that supervisors shall perform their work by reporting on their sections to the next higher supervisor, and so on. In the case of the factory, by virtue of its small size, much of this reporting was done personally, but in the case of the air force organization, a large amount of paper work is created by this requirement and the specter of SOP⁷ is everywhere in evidence. Two consequences tend to occur: first, a fairly elaborate "covering-up" process, whereby higher supervisors were sometimes almost isolated from their work sections and were thus only partially aware of their degree of success in reaching organizational goals, and second, a tendency for persons to get buried in paper-work to such an extent that they were able to spend only a little time actually coordinating their departments.

At the air site, the informal organization related itself to these institutional concomitants as follows. Preliminary findings suggest that in addition to cliquing between specialties, there is also a considerable tendency for cliques to be confined to a given specialty. We are working now on the hypothesis that this phenomenon is related to the work section's ability to accomplish its mission without formal direction or in spite of ineffective direction. There is some evi-

dence that the work section may be operated informally. The paper work problem is in part solved by informal sessions—one commanding officer sees to it that officers report to the club daily for coffee—where experiences are traded and higher-ranking officers gain knowledge of what is taking place in their work sections. This serves also to protect them should paper reveal inadequacies; they have at least been forewarned. It seems to serve the further function of providing opportunities for supervisors to evaluate one another's capabilities and thus develop confidence in one another.

This leads us to the third institutional control; namely, the selection system. At the factory, this is handled in the usual manner by a personnel office. Here, the individual's qualifications are examined and a decision reached concerning whether he can perform the work role for which there is a vacancy. In the Air Force, as in military organizations generally, entrance is facilitated, but the individual is then given a set of tests to determine aptitude and interests. Persons are then sent to a school (if necessary) in large numbers to equip them for jobs to which they are later assigned. Theoretically, if persons have been well-trained and have appropriate interests and aptitudes, the organization should succeed in filling its vacancies in this manner.

What is noteworthy about the approach in both instances is that it focuses attention completely on the individual and his abilities, and ignores completely the cooperative nature of the organization in which he must play a role. The individual may have the requisite skills, but before he can play the role successfully, other workers must be willing to accept him in that role. At the factory, it was discovered that employees engaged in a continuous process of mutual evaluation in terms of the following criteria: personal characteristics, marital ties, religion, education, race, ethnicity and language, experience, and union membership.⁸ There were sentiments expressed that these characteristics made a difference in terms of the amount of respect or confidence one should have in fellow workers, and that the formal organization did not sufficiently reward pos-

⁶ See Warner, W. L., Meeker, M. and Eells, E., *Social Class in America*, Chicago: Science Research, Associates, Chap. 1, for a discussion of the functions of supervision.

⁷ Abbreviation for Standard Operating Procedure.

⁸ For a full discussion, see Chapter 3 of the reference cited in footnote 2 above.

sessors of what were considered desirable characteristics. Said the secretary to the office manager:

"You know, there are quite a few refugees from Nazi Germany here. They don't seem to get along so well in the office. They seem to be set in their ways. I don't know what it is, but they just form a group apart. Fred (her husband) says it's because of their background. Most of them have never worked before in their life. They don't fit into our industrial system at all. They just don't seem to have the knack. Many times, Mr. Hanson (her supervisor) and I are going over a form and I see immediately what the trouble is, but he has to go slowly and ploddingly around, taking small steps, until he eventually comes to the same conclusion that I do. But he has to do it in the roundabout slow, methodical way."

Another worker, commenting on the problem of trying to work with Mr. Hanson said, "Talking to Hanson is like talking to yourself."

Cliques, by their very nature, tend to be made up of persons who feel at ease in one another's presence and feel confidence in one another. Indeed, it is through clique experience that such confidence may be highly developed. As the secretary to a vice-president explained:

"A private secretary is the top of the heap. You need something else besides the ability to type and take shorthand. You've got to feel you're working for the company and not just for yourself. Now Mildred and Emma (other private secretaries)—we see eye to eye on that. Louise—she's a good little stenographer, but she'll never be a secretary. She doesn't fit into our crowd. When we go out for coffee, she tags along. Then she'll usually complain about her boss. She can't accept the idea that you don't work for a boss, you work for the company."

In the air force organization, the following situation prevailed. The air site, like all military organizations, is required to make best use of the men assigned to it, whether they have what are regarded as desirable qualities or not. Since a large proportion of the personnel are either draftees or involuntary recalls,⁹ one of the most important criteria for

⁹ A considerable proportion qualify as "volunteers," but on questionnaires gave overwhelmingly as their reason for volunteering: "To avoid getting drafted into the Army."

success in a work role is likely to be an identification with the Air Force and the mission of the squadron. The implicit question arises: Where do you feel you belong? Are you one of us, or do you still regard yourself as a civilian? The problem is complicated in surveillance work for two reasons. First, there exists a split between personnel with and without wings. Ability to fly is not related to the mission of the site; yet those with wings exhibit a tendency to identify with the flying air force outside the site. Second, not all radar personnel desire to remain in that work after their one year AFSC requirement has been met.

There is some evidence that the informal organization is used to discover primary identifications and also to change those identifications. Thus, the commanding officer at one site organized a duty night, which, after two hours of work, turned into a stag party till 3:00 a.m. There were later objections from the wives of officers present, about which the commanding officer said in an interview:

"I called that a duty night because that's exactly what it was. There's too much of this dashing for the gate right after a man's shift is over to get home, or to get in flying time. I wanted to take this means of informing my men that radar comes first, family and flying second. If I, as their CO, want them to stay till 3 in the morning to work, they'll do it. If I want them to play poker till 3, they'll do it, because I said so. Let the wives gripe. It's time they found out their husbands are in the Air Force."

It was significant that the commanding officer employed an informal means of driving the point home, for the formal regulations are quite clear on this point. The point here is that the selection system does not insure a supply, either in the office or the Air Force, of persons who have what are regarded as "desirable" characteristics by persons within the organization. In the case of both the office and the air site, primary control devices are employed both to discover these characteristics and to try to develop them if they are absent.

Evidence has been submitted which suggests that primary controls on behavior are far from being inconsistent with institutional controls. There is an unfortunate tendency in the literature on the sociology of work

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to regard cliques as innocuous play groups, or else as being antithetical to the purposes of the organization, as exemplified in restriction of output.¹⁰ While this is unquestionably the case in some situations, it is suggested here that cliques may have quite another purpose—they may actually be es-

sential to the very functioning of instituted organizations.

The data presented here, based as they are on only two work organizations, are inconclusive. But they do suggest that the relation between primary and institutional controls is by no means a simple one, and that further research on this relation may prove fruitful in the examination of behavior in formal work organizations.¹¹

¹⁰ Cf. F. J. Roethlisberger and W. J. Dickson, *Management and the Worker*, Cambridge: Harvard University Press, 1947, Chapters XXII and XXIII, and W. E. Moore, *Industrial Relations and the Social Order*, New York: The Macmillan Co., 1947, Chapter XV. An exception to this emphasis is provided in D. C. Miller and W. H. Form, *Industrial Sociology*, New York: Harper and Bros., 1951, Chapter IX.

¹¹ See also W. F. Whyte, *Street Corner Society*, Chicago: University of Chicago Press, 1943, for a discussion of the significance of informal organization in the slum, which deals with roughly the same theme as that discussed here, though in a different social situation.

ROLE CONFLICT AND AMBIVALENCE IN LEADERSHIP

MELVIN SEEMAN

The Ohio State University

THERE is a growing awareness that the study of leadership cannot be adequately conducted without reference to the cultural situation in which that leadership takes place. This trend is part of a general shift away from the traditional psychological "trait" approach, in favor of an approach which stresses situational variations in leadership. Robert Nisbet has commented along these lines that "there is a quickly reached limit to both the theoretical value and the practical utility of studies in human relations that lose sight of the historically given institutional realities of our time."¹ This emphasis on the importance of the "total cultural" situation is especially congenial to the sociologist; but one of the

real problems is the implementation of this view in research. The purpose of this paper is to report on an empirical investigation which arose from such a total cultural view; namely, an investigation of the significance of role conflict in leadership.

Role conflict here refers to the exposure of the individual in a given position to incompatible behavioral expectations. Though an apparent incompatibility may be resolved, avoided, or minimized in various ways, the conflicting demands cannot be completely and realistically fulfilled.² Ambivalence is the subjectively sensed aspect of this conflict, or, from the behavioral point of view, the validation in behavior of the fact that the actor experiences difficulty of choice in the performance of given behavior alternatives.

¹ "Leadership and Social Crisis," in A. Gouldner (ed.), *Studies in Leadership*, New York: Harpers, 1950, p. 708. As is amply documented in this volume, the situationist approach has indeed won the day; but it is important to recognize the differences among the various brands of situationism, "the total cultural" brand being only one of them. The writer has tried elsewhere to describe three current situational approaches and to examine the research which flows from them; cf. "The Ohio State Leadership Studies: Some Methodological Issues in Leadership Research" in the report of the Seventh Annual Conference on Research in Industrial Relations, sponsored by the Labor Market Research Committee of the Social Science Research Council, June 1951.

² This view is in effect a paraphrase of the definition of role conflict found in Talcott Parsons, *The Social System*, Glencoe, Illinois: The Free Press, 1951, p. 280. The term "role conflict" may be somewhat misleading, carrying implications of necessary personal conflict. This refers, however, only to situations in which the observer notes what appear to be conflicting sets of expectations—i.e., to *potential* sources of difficulty for the actor. The ways in which such potential conflicts are handled so as to avoid ambivalence are not here the subject of study (e.g., ruling out the legitimacy of certain expectations; appealing to higher values; etc.)

The gist of the argument which follows can be summarized in three propositions.

1. The empirical evidence supports the notion that institutional leadership positions are positions of high vulnerability, in the sense that our cultural imperatives impose mutually contradictory demands with which the institutional leader must deal. At least four major bi-polarities, or choice points, of special importance for leadership can be isolated, these polarities being a significant part of those "institutional realities" of American culture which must be kept in mind as backgrounds for the understanding of leader behavior. There is no implication that these polarities are exclusive to American culture, though their order of importance and their specific manifestations are in all likelihood unique.
2. The specific role conflicts associated with these polarities can best be seen not simply as a single general category, but are analyzable into *types* of role conflict which have differential meaning for leaders.
3. There is evidence that these role conflicts, derived initially from the standpoint of the observer as *potential* sources of conflict, are in fact responded to as such by the actors in the situation. Our respondents reveal the self-contradiction and decision difficulties that a role conflict analysis of the situation would lead one to expect.³

DIMENSIONS AND TYPES OF ROLE CONFLICT

The writer has indicated that the concern with role conflict grew out of an appraisal of American culture. As others have recently noted, the attempt to characterize typical national characters or typical cultural values involves serious theoretical and methodologi-

cal difficulties.⁴ The major cautions are, perhaps, that we conduct such an investigation without becoming involved in unilateralism (e.g., the child disciplines explanation), and without getting involved in a kind of naive cultural determinism, which tends to reify the observed cultural "themes" as entities, and purports to show how the widest possible range of social behaviors are derivatives of, or emanate from, this constructed model of cultural reality.

In the present case, the aim is to use an analysis of the "total cultural situation" as a guide to significant variables whose operation in leadership must be further examined, in the hope, of course, that such a procedure may yield findings which touch upon basic issues in American social life. A review of the available experimental literature on leadership suggests that such findings are not the typical product of the "isolationist" tradition—that tradition which takes the leader and his immediate group as the given for examination. Data were obtained bearing upon four major bi-polarities of value, or dimensions of role conflict. These dimensions are labeled and briefly described below.

Certainly one putative polar characteristic of American culture (and perhaps the most agreed upon) involves *the status dimension*. This refers to the conflict between the success ideology on the one hand, and the equality ideology on the other. These two ideologies, taken to be in some degree incompatible, lead some persons to honor individually achieved personal success and at the same time to deny the existence and significance of differences in status. There are presumably important ambivalences about leadership in America which are understandable in the light of this dual ideology about status; and, indeed, we see around us evidences of such ambivalence—the persistent halo that surrounds "leaders" and great men in America; the sneer directed at those in the army who were "bucking for promotion," and the like. In a variety of contexts, *The American Soldier* provides illustrations of

³ The data on which this paper is based were gathered in a study of leaders in school systems (superintendents and principals), and the term "leader" throughout refers to individuals occupying such institutionalized positions of leadership. The study was conducted in 26 randomly selected communities in Ohio, the communities having been chosen from a parent population of middle-sized Ohio cities. "Middle-sized," in this instance, means large enough to have a full-time non-teaching superintendent, and small enough to have no official intermediate administrative staff (e.g., assistant superintendent, curriculum director). The communities range in population from 4,500 to 15,000.

⁴ One of the best of these recent commentaries is the work of Gabriel Almond, *The American People and Foreign Policy*, New York: Harcourt, Brace, 1950. Another, and more critical review is Morroe Berger's "Understanding National Character and War," *Commentary*, 2 (April, 1951), pp. 375-386.

this kind of ambivalence, or "leadership guilt." The volume notes, for example, that "there can be little doubt that attitudes toward officers represented a generalized attitude toward a system of special privilege alien to democratic civilian folkways";⁵ but criticism of the system existed side by side with a desire to achieve status within the system. And at one point, M. Brewster Smith, describing the experience of Officer Candidate School, says:

The new officer, somewhat insecure in his role and perhaps a little guilty at his favored status over his previous enlisted confreres, reactively asserts his status, and finds in the OCS ordeal a justification for his new prerogative; he *earned* them.⁶

A second area of role conflict involves *the authority dimension*. It is the conflict between the values of dependence and independence; and it is, perhaps, most thoroughly described in a theoretical way by Erich Fromm in terms of modern man's *Escape from Freedom*:

Freedom, though it has brought him independence and rationality, has made him isolated and, thereby, anxious and powerless. This isolation is unbearable and the alternatives he is confronted with are either to escape from the burden of this freedom into new dependencies and submission, or to advance to the full realization of positive freedom. . . .⁷

There are disagreements, of course, about whether such a conflict exists and whether it exists in any differential degree for Americans as compared with others. If there is any merit, however, to the recent description of *The Authoritarian Personality*,⁸ the implication is plain that the problem of authority, of strength and weakness, is a source of considerable ambivalence for large segments of the American people. How such ambivalences, if they exist, are reflected in specific leader-follower relations is the task of empirical analysis; and such an analysis should throw much light, for example, on the opera-

tion of "non-directive" procedures, in the clinic or in group leadership.

The third conflict of special relevance for leadership, labeled for shorthand purposes as *the institutional dimension*, involves the choice between universalist as against particularist criteria for social action. Stouffer and Toby have reported empirical work along these lines, and in so doing they comment:

An especially common role conflict is that between one's institutionalized obligations of friendship and one's institutionalized obligations to a society. . . . A universalistic obligation is applicable to dealings with anybody (e.g., obligation to fulfill a contract); a particularist obligation is limited to persons who stand in some special relationship to one (e.g., obligation to help a relative or a close friend or neighbor).⁹

The official leader, holding in the usual case a key position affecting the distribution of reward and punishment is, hypothetically at least, highly likely to become involved in this kind of conflict precisely because of his combined power and office.

The final major area of conflict involves *the means-ends dimension*. It is the conflict between emphasis on getting the practical job done as against emphasis on the process of achievement. Almond comments on the presumably widespread belief in America in the compatibility of morality and expediency, and on the pendulum movements between emphasis on the two.¹⁰ A large share of the literature on "group dynamics" reflects a concern with this area of conflict in its discussion of relative emphasis on group process and group product. And in a more empirical effort in our own research organization, two major factors accounting for variation in the description of leader behavior have been isolated.¹¹ The two factors are called "initiating structure" and "consideration"; and they again suggest that one of the critical problems is that of resolving the conflict between being a leader in the

⁵ S. A. Stouffer and J. Toby, "Role Conflict and Personality," *American Journal of Sociology*, 56 (March, 1951), pp. 395-406.

¹⁰ G. Almond, *op. cit.*, pp. 51 ff.

¹¹ J. K. Hemphill, *Leader Behavior Description*, Personnel Research Board, Ohio State University, 1950 (mimeographed); A. W. Halpin and B. J. Winer, *The Leadership Behavior of Airplane Commanders*, Technical Report, Personnel Research Board, 1952 (mimeographed).

⁶ S. A. Stouffer, et al., *The American Soldier*, Princeton University Press, 1949, Vol. 1, p. 382.

⁷ *Ibid.*, p. 390.

⁸ *Escape from Freedom*. New York: Farrar and Rinehart, 1941, p. vii.

⁹ T. W. Adorno, et al., *The Authoritarian Personality*. New York: Harpers, 1950.

achievement sense of the word (i.e., attaining, directing), and at the same time maintaining an adequate *process* of achievement (i.e., sound interpersonal relations, good morale and the like).¹²

What are the *types* of role conflict in which these four dimensions of conflict are manifested? The typing of role conflict is based upon the situation that obtains with regard to the criterion group or groups—i.e., those significant others who are the definers of the social role. One of these types of conflict is characterized by *agreement within the criterion group* on behaviors which are mutually difficult to achieve under the given institutional conditions.

An empirical illustration of this type of conflict comes from our study of school systems. Teachers were asked to describe the superintendent's leadership using pre-tested scales measuring, among other behaviors, the leader's degree of "separatism." The separatism score is essentially a membership or social distance measure, describing the extent to which there is informal interaction between the leader and group members. In addition, a wide variety of objective data on financial trends in these same schools and communities was obtained.

For present purposes, one of the most striking results was the positive correlation of .40 (significant at the .05 level for an N of 26 communities) between the superintendents' "separatism" (as described by the teachers) and the amount of salary increase obtained for the teaching staff over a three year period. In short, where salaries went up there was high leader separatism.

If we assume, as our evidence on superintendent evaluation suggests, that the prevailing teacher code condemns superintendent separatism, and, of course, approves salary increases, we find apparently that the superintendent is placed in the unenviable posi-

tion of being asked to engage in two behaviors which do not "go together." In order to achieve salary increases, he presumably must spend his time not with subordinates, but with those superiors and community influentials who wield power. As one superintendent succinctly put it: "You don't visit your classrooms regularly because you're writing publicity for the next levy that you can't have fail. It must pass. Therefore, you don't know what's going on in the classrooms too well."

Yet the normative code, it seems, asks him to do both, and though the trend is against it, some superintendents do succeed in achieving both salary increases and low separatism. It is interesting to speculate on the toll which such success exacts in mental health or in long run administrative efficiency. That there are such tolls is not a matter of pure speculation, for the sense of tremendous pressure as a result of joint school and community demands runs through many of the interviews with these same superintendents.

The major point, however, is that here is a case of role conflict characterized by substantial agreement within the criterion group in imposing contradictory role demands. And it is also, from the standpoint of the discussion of dimensions of conflict, a situation in which economic *ends* and primary group *processes* of solidarity and intimacy are both defined as goods in an institutional setting which makes both difficult to achieve.

A second type of role conflict involves significant *disagreement within the criterion group* regarding role definition. In the school study, for example, the teachers were asked ten forced-choice questions concerning the role of an "ideal superintendent." The alternatives for choice reflect the dimensions of role conflict described above. Two illustrations will perhaps indicate how the results exemplify this second type of conflict and document another dimension of conflict.

One question asked, "Should an ideal superintendent invite staff members to his home for social occasions?"; and a second, "Should an ideal superintendent feel free to discuss personal problems of his with the teachers?"; Simple "yes" and "no" answers were the forced-choice alternatives provided. In both cases, the split in opinion among the teachers was close to a 60-40 division.

¹² Before proceeding to a discussion of the types of conflict, a word is in order about the problem of derivation and overlap of the four dimensions. As to derivation, the four dimensions are not meant to be an exhaustive list comparable to Parsons' pattern-variable dichotomies, but are simply four major choice points which were deemed especially relevant to the leadership problem and on which empirical data were obtained. Whether the four dimensions reviewed can be considered as independent variables remains to be empirically demonstrated.

Taking all of the teachers together, without regard to community differences (an N of 500 teachers in the 26 communities), 61 per cent said that the leader should invite teachers to his home for social occasions while 39 per cent felt that he should not; and, on the second question, 65 per cent responded that the superintendent should not discuss his personal problems with subordinate staff members.

We have here a reflection of the conflict between universalist and particularist role demands, with the conflict in this case being a within-criterion-group type. The answers reflect conflict in the institutional dimension since the disagreement centers around the question of whether or not the official leader will be drawn into "favoritism" and "personalistic" dealings if he engages in these behaviors which lie outside the official institutional definition of his role. Thus, the leader, in attempting to deal with this situation, is not simply faced with certain minor disagreements of policy represented in the manifest content of the items reported, but is coping with problems which have a wider cultural significance.

We should note at the same time that the within-group type of conflict reveals itself in the other conflict dimensions as well. On the status issue, for example, one question asked whether the superintendent should "generally act as chairman of group meetings (total staff, grade meetings, committees)." The split on this issue was 63 per cent answering "yes" and 37 per cent answering "no."

The recognition of all this may seem obvious to some, but it is certainly not obvious to those who are working in the situation. We asked these superintendents, in interviews conducted one year after the original data collection, what they thought the significance of these opinion discrepancies was and what should be done about them. For the most part, the answers constituted puzzled evasions—some literally avoided the issue by passing quickly on to other matters, some took refuge in a purely statistical definition of appropriate action ("just go along with the 60 per cent"), and some, expressing pleasure in the fact that such differences of opinion were democratically possible, insisted that nothing needed to be done and left it at that. In our own view, the training impli-

cations which stem from these results, seen in the conflict dimensions and conflict types perspective, go far beyond the "human relations skills" conception employed in the typical leadership training program.

A third type of role conflict involves *disagreement between criterion groups* regarding the nature of the given role. It is to situations of this type that the literature on "role conflict" typically points.¹³ One of the most important of such criterion group disagreements, in our data, arises out of the different definitions of leadership role by the subordinates as compared with the leader group themselves. The area of conflict involved is one we have labeled as the authority dimension.

Both the leaders and the teachers were asked the following questions:

1. Where a student's passing or failing is doubtful, do you think an ideal superintendent should:
 - a. leave the decision up to the teacher?
 - b. pretty much take responsibility for the final decision?
2. Should an ideal school leader work out school problems:
 - a. with a maximum of efficient personal leadership by him?
 - b. with a maximum of staff participation in the decisions that are made?
3. Should an ideal school leader:
 - a. fit his ideas into a group discussion in about the same way as other members of the group?
 - b. tell the group at the outset what his ideas on the given subject are?

In all three cases one of the choices offered defines the role of the school leader in a relatively directive, "true leadership" fashion. And in all three cases, significant differences were obtained between the choices of the leaders and the teachers. On the first item, only 40 per cent of the leader group chose the alternative "leave the decision to the teacher" as compared with 80 per cent of the teacher group; on the second item, 5 per cent of the leader group as against 17 per cent of the teacher group selected the "efficient personal leadership" choice; and on the

¹³ The classic example of this kind is the "man-in-the-middle" view of the foreman in industry, seen as one who is caught between the conflicting definitions of his role by top management and by his subordinates.

third item, 7 per cent of the leaders but 20 per cent of the subordinates chose the "tell the group at the outset" alternative.¹⁴

All of these items deal, in some form, with the question of the allocation of responsibility, that is, with the dependence-independence dimension. In this connection, three facts are especially worthy of note. There is, first, the fact that on some responsibility items the majority opinion of leaders and teachers is antithetical, with the teachers demanding more responsibility than the leaders are prepared to give. Second, even where there is majority agreement between the two groups, there is nevertheless a significantly greater minority of teachers who are in effect asking the superintendent to assert authority in situations where the leaders are denying that authority. Third, we should note that the superintendents frequently complained, in the recorded interviews, that their efforts toward democratic staff participation were blocked by the unwillingness of staff members to take the responsibility that goes along with decision-making power.

Though this latter complaint may be, in some part, simply a rationalization for inertia or personal power, these facts taken together nevertheless are indicative of the fact that the role conflict along the dependence-independence dimension has its reciprocal sides for leader and follower. The subordinate is caught in the dilemma of wanting not to be bossed, but at the same time wanting decision responsibilities to be borne by some other. The leader is, by the same token, caught between what we might call the authority imperative and the non-directive imperative.

THE PROBLEM OF AMBIVALENCE

In sum, we have been arguing that an analysis in terms of dimensions and types of role conflict places the problem of leadership styles in its more complete and realistic cultural context. Thus far, we have dealt only

with role conflict from the standpoint of the observer viewing what, from the outside, appear to be dilemmas of action for leader and follower. Is there any evidence that these are more than logical contradictions, evidence that the actors themselves reveal the kind of ambivalence we would expect to flow from these contradictions? Interview and questionnaire data indicate that the language of the actor, so to speak, betrays and at times directly expresses the conflicts discussed above.

We have suggested, for example, that the office leader places himself in a uniquely vulnerable position, given the traditional American ambivalences about equality and hierarchy. One of our superintendents reflects this basic duality in his comment:

We shouldn't fear our superiors, and yet I do think that there should be respect. We shouldn't have contempt for a person just because he's a superior.

The juxtaposition of contempt and superiority is symptomatic of those status dimension difficulties to which we referred. A second superintendent reveals the status conflict in another way, namely by denial and assertion of its worth within the same interview. In the early section of an interview lasting more than an hour, he made the following comment about his position:

It carries a tremendous social prestige and professional prestige, and I'm sometimes annoyed—well, I have a Ph.D.—I'm sometimes annoyed because I never use the title myself, but they're always careful to. I don't know why. It's an annoyance to me because it's unimportant to me personally. Social lines are pretty well drawn in the community. I'm very much puzzled every once in a while by running into that experience where they (the teachers) would prefer to have me out in front, which I don't particularly want to be.

At a later point in the interview, when asked, "What are the positive things associated with your job?", his first remark was:

When I think about it, there's a lot of satisfaction because of the prestige the position of superintendency carries in this community. I've been amazed at times how just a word here or there will carry tremendous weight. It will with factory management, for instance, and it will in business circles. I've had a lot of satisfaction from that. . . .

¹⁴ In these illustrations, the number of leaders (superintendents, high school and elementary school principals) is 77; and the number of teachers is 1065. The percentage discrepancies indicated are not the result of wide discrepancies in only a few schools, since the pattern is quite similar from system to system and between schools within a given system.

On the universalist-particularist dimension, there is again evidence in the interviews that this is a very real problem. One superintendent expressed the indecision in this way:

I don't think though—in my honest opinion—that an administrator should bring himself down to the level in all cases; of course I do think we're all teachers. But, at the same time, if you become one of them maybe you don't always accomplish. Yes, but maybe you do. I don't know. In fact, I have several (teachers with whom) my wife and I are very good friends.

At a later point, this same leader commented:

You can become too—maybe too—putting yourself on too much of an equal—to be too friendly. Now there has been some little thought that has come to my mind that maybe I have been a little too friendly.

Though one might look at such comments in their purely status aspects, the superintendents in this connection are chiefly concerned about the confusion of universalist and particularist criteria. As another leader put it:

I mean, they would feel, "He's an old buddy of mine." If you had to crack the whip a little bit, or set down some rule, then they'd be offended quicker.

On the authority dimension, there are many evidences that retreat from responsibility, as noted earlier, is a problem. Two superintendents commented as follows:

I think it's your own group and your own people that seem to imply or seem to infer that you just carry on and take care of everything.

The limitation on democracy in school administration is this: there are a great many teachers who do not want that participation because of the responsibility that goes with it.

That the issue, however, is not as clearly one-sided as this is indicated by further remarks made by these same superintendents. One, in talking about the question of "staff participation" versus "efficient personal leadership," said:

You're not showing very much leadership if you follow. Your experiences should train you to have a strong point of view—and not be influenced by the group. Course, there is

the other side of the picture. Two or three or four minds or opinions, if they're good minds, always strengthen a point, if it's reasoned properly. But here lately I'm having considerable difficulty in my thinking on that. . . . In other words, your staff seems to think that your leader should fall in line. . . . That's a weakness to my notion.

Finally, on the means-ends problem, one of the critical leader difficulties lies evidently in finding a clear path between their ends-in-view and the processes of staff participation, and frequently out of this difficulty comes a disturbingly clear Machiavellian view of the situation. Thus, one superintendent remarked:

I think you will find that you can lead them around to the place where they will see things as you want them. . . . You know, after all, you appoint your committee, and you have key individuals on your staff that you use as chairmen, and those chairmen feel pretty much as you do. At least you talk it over with them. And you control your committee action through your chairmen that you appoint.

Approaching the problem of ambivalence from a more statistical viewpoint, we asked our sample to indicate how difficult each forced-choice on ideal leadership was to decide upon. Degree of difficulty was rated on a four-point scale. The corrected split-half reliability of these ambivalence scores based on ten items was .87. The measure is admittedly a crude one and does not warrant detailed examination here, where we have been concerned largely with the development and description of a role conflict scheme, but it is adequate perhaps to indicate the profit of examining the leadership problem in these terms.

One finding of particular interest, for example, is presented in Table 1. The evidence clearly indicates that the leaders themselves achieve a consistently lower ambivalence score than the teachers, that is, superintendents describing the ideal superintendent role have a 2.92 ambivalence score as compared with teachers describing this same role who have a score of 3.35, and so on. The mean ambivalence score for all leaders is 2.68 and for all teachers 3.29, a difference which is significant at the .01 level.

Though there are other entertainable hypotheses to account for these differences

TABLE 1. MEAN AMBIVALENCE SCORES OF SCHOOL LEADERS AND TEACHERS, BY POSITION DESCRIBED

	Superintendent	N	High School Principal	N	Elementary School Principal	N
Leaders	2.92	26	2.33	27	2.79	24
Teachers *	3.35	503	3.34	327	3.15	216

* Teachers holding different positions in the school system were asked to describe the "ideal leader" relevant to their position, e.g., the high school teacher describing a high school principal.

(e.g., the factor of experience in the role), one interpretation would suggest that these data point again to the high vulnerability of the leader role. What we do, in effect, is to place the leader in a position of built-in conflict, and then demand of him a greater clarity and decisiveness regarding that role than we ourselves command. This view regards the low scores of the leaders as responses to the pressure for such clarity—that is, the leaders respond as "real leaders should," namely with decision and conviction. Part of the evidence that this may be the case is found in the frequency with which, in the more intimate and informal interview situation, the leaders said that decisions previously rated "not very hard" were, in fact, difficult to make.

Whatever the validity of such an inter-

pretation, it does highlight the possibility of, and need of moving toward, an analysis of the *consequences*, for person and institution, of the role conflict and ambivalence problem. Two kinds of consequences are immediately suggested by the example above: What difference does it make, in teacher morale, school efficiency, mental health and the like, if the leader is aware of these role pressures or adopts particular modes of adjustment and resolution; and secondly, what kinds of training, of leader and subordinate, would be required to effect optimal change in the system as now constituted? The scheme which this paper has presented is intended to make the asking of such questions about consequences more analytically possible, and to place these questions in the perspective of a particular cultural setting.

A NOTE ON PARTICIPATION IN VOLUNTARY ASSOCIATIONS IN A MEXICAN CITY *

FLOYD DOTSON

University of Connecticut

THERE is increasing awareness among sociologists and anthropologists of the need for comparative studies of urban societies. To what degree does their unique interactional situation—the close physical proximity of inhabitants with a wide range in their internal differentiation and social

distance—make all urban communities alike? What range of cultural variation is possible within the structural uniformities of cities? These are important problems which are beginning to receive attention.¹

The connection between urbanization and the development of voluntary associations is a special case in point. It has long been accepted as axiomatic that the growth of cities in the United States and the voluntary associations which are such an outstanding feature of American social structure are directly

* Some of the data here reported appeared in a paper ("Voluntary Associations and Urban Social Structure") presented by proxy in a Spanish translation at the Second National Congress of Sociology, Guadalajara, Mexico, October, 1951. The case material has since been reclassified, and so there are slight discrepancies between certain of the figures used at that time and in the present paper.

¹ Cf. Ralph L. Beals, "Urbanism, Urbanization, and Acculturation," *American Anthropologist*, LIII (January-March, 1951), pp. 1-10.

related phenomena.² But the development of formally organized voluntary associations in other urban cultural areas remains largely unexplored.³ Within this context, the small pilot study reported in this paper has significance in spite of the limited scope of the research. The data are drawn from an inquiry into the interpersonal relationships⁴ of 415 adult residents of Guadalajara, Mexico's second largest city.⁵

THE RESEARCH PROCEDURE

A major purpose of this project was experimental—to test the feasibility of house-to-house interviewing of a Latin American urban population as a step in the planning of further research in the area.⁶ Lack of sufficient background data complicates sampling procedure in Mexico and the fieldwork had to be fitted into a nine-weeks schedule. For this reason a proportional sample of the general population was not attempted. The purposes of an exploratory study would be

² Recent studies indicating a great deal of non-participation among the lower classes have modified rather than destroyed this generalization as it applies to the American scene. See the author's "Patterns of Voluntary Association Among Urban Working-Class Families," *American Sociological Review*, XVI (October, 1951), pp. 687-693, for further discussion of this point and bibliography.

³ There is nothing approaching a systematic study of voluntary associations in the existing literature on Latin American urban communities. A number of writers have of course touched upon the subject tangentially in general descriptions of social structure in the area. See, in particular, the series edited by Theo R. Crevenna, *Materiales para el estudio de la clase media en la America Latina*, Washington, D. C.: Office of the Social Sciences, Pan American Union, 1950-51, 6 volumes.

⁴ The study also included participation in clique and friendship groups, with particular emphasis upon the role of kinship in the determination of these relationships. The field-work was done in the summer of 1951.

⁵ Guadalajara, capital of the state of Jalisco, lies near the rim of the central plateau, some 400 miles west and north of Mexico City. It had a population of 378,000 in 1950.

⁶ The rate of refusals was about 20 per cent, except for the upper classes, where it was higher. Among the upper classes, it was usually necessary to send in a card and to explain what was wanted through a servant (or a series of servants) and refusal was thus made easier. When personal contact could be established with the husband or wife of upper-class households, interviews were as easily obtained and rapport was as good as in the other classes.

adequately served, it was decided, if cases were drawn from all class levels and if income data were collected to specify the socio-economic characteristics (insofar as these are measurable by a single index) of this particular sample.

Interviews were obtained by visiting a series of contiguous households on a different street each day. In this way a random selection of particular cases was approximated, although a conscious attempt was made to get cases from all social classes by choosing streets in low-, intermediate-, and high-income areas along which to work. Where the household visited was occupied by more than one nuclear family, only the recognized head of the household or his spouse was interviewed. Interviews were obtained from members of a total of 231 households. Questions on voluntary association membership and activities were limited to the respondent and the husband or wife of the respondent, if he or she were living. No attempt was made to obtain data on children's activities. Data were obtained in this manner for 415 adult individuals—230 women and 185 men. Among these are 184 married couples. The predominance of women is due mainly to the 31 widows among our respondents. The other persons are divorced, separated or unmarried, but are the heads of the households visited.

Each informant was asked to estimate the total income of the household unit to which he or she belonged. (Where the house was occupied by two or more complete nuclear families, income data for only the family of the person interviewed were included.) The distribution of households by monthly income (in pesos) is shown in Table 1.⁷

⁷ At the time this study was made, the exchange rate was 8.65 pesos to the dollar.

Some examples of incomes reported by our respondents may make Table 1 more meaningful to the reader. Unskilled laborers on "pick-and-shovel" construction jobs earned from 6 to 10 pesos a day in 1951. A good automobile mechanic may make 500 pesos or more a month. Women elementary school teachers are paid between 200 and 250 a month. Government clerks have similar incomes. Many white-collar workers have more than one job; a bookkeeper among our respondents brings his monthly earnings to 1600 pesos by teaching in three different commercial night schools in addition to his regular job. Business and professional incomes of course vary greatly. One of the three doctors in our sample reported an income

TABLE 1. HOUSEHOLDS BY MONTHLY INCOME

Monthly Income in Pesos	Number of Households	Percentage of Households
Under 250	29	12.6
250-500	57	24.7
500-1000	64	27.7
1000-2000	51	22.0
2000-3000	17	7.4
Over 3000	13	5.6
Total	231	100.0

Statistics showing detailed income distribution are not available for Guadalajara (or, to the best of the author's knowledge, for other Mexican cities). For this reason it is impossible to give an accurate comparison of our sample with the general population. It is safe to say, however, that compared to the general population, this sample is considerably over-represented in the middle- and upper-income brackets.

ASSOCIATIONS REPRESENTED IN THE SAMPLE

The associations which are represented by at least one membership in the sample studied may be grouped with a minimum of distortion into 10 type-categories.

Church-affiliated societies. From the standpoint of membership, the most important voluntary association connected with the Church in Guadalajara is the Catholic Action. The Catholic Action (established in Mexico in 1927) is a lay organization which, ideally, promotes Catholic standards of behavior in all areas of social life. It is subdivided into appropriate age, sex, and marital-status groups, each of which virtually constitutes a separate association. In addition to the Catholic Action, there are a considerable number of devotional organizations founded for the veneration of a particular saint or virtue, or for the performance of a specific religious task. Some of these *cofradías* and similar pious associations are very old, having a history which goes back

of 800 a month, the other two between 1000 and 2000, which is probably the modal range for general practitioners. Among our cases in the highest income category (3000 pesos and over) are the owner of a large construction company, the owner of a pharmaceutical and cosmetics factory, and the manager of a leading department store.

to the Conquest period.⁸ There are also a few non-devotional special-interest organizations of recent origin, such as parents' clubs and the Knights of Columbus.

Athletic and sport clubs. There are three major athletic clubs in Guadalajara, with facilities for ball games, tennis, and swimming. In addition to them there are many small, special-interest organizations—those for cyclists, marksmen, and hikers may serve as illustrative examples.

Labor unions. Unionization has been encouraged by the Mexican government since the Revolution, consequently, membership in unions by both manual laborers and white-collar workers is common. However, unionization is by no means universal—construction workers, to take a notable example, are still only partially organized. As in the United States, membership in a union for most Mexicans appears to be nominal rather than active in terms of actual participation.

Society clubs. Under this rubric are included associations of varied activities for both sexes. But they have in common the fact that their appeal is partly, if not entirely, "social" in nature, and they are exclusive in membership.

Professional and learned societies. Organizations are more numerous in medicine than in any other field, but all of the established professions seem to be represented by at least one association.

International service clubs. These are represented in Guadalajara by the Lions, the Rotary, and a recently organized Twenty-Thirty Club. Their organization and professed aims follow those of the parent-associations in the United States.⁹

Mutualist societies. These organizations, which are related in origin to the mutual aid and insurance societies of Europe and North America,¹⁰ combine in Guadalajara the ac-

⁸ Associations of this kind, which stem from medieval prototypes, are not confined to Latin American Catholicism, but they seem to have been uniquely elaborated there. See Oscar Lewis, *Life in a Mexican Village*, Urbana: University of Illinois Press, 1951, pp. 263-269, and John Gillin, "Modern Latin American Culture," *Social Forces*, XXV (March, 1947), pp. 243-248.

⁹ Cf. Charles F. Marden, *Rotary and Its Brothers*, Princeton: Princeton University Press, 1935.

¹⁰ W. H. Dawson, "Mutual Aid Societies," *Encyclopedia of the Social Sciences*, New York: Macmillan Co., 1933, vol. 11, pp. 168-172.

tivities of athletic and social clubs with insurance or death benefits for the members. There are several classes of membership. Some members use the facilities of the clubs at a reduced fee without participating in the insurance programs.

National and local business and professional men's clubs. Similar to the international service clubs but of Mexican origin are several associations for business and professional men. Some of these are national in scope, others are purely local.

Ethnic and regional societies. Of the two societies of this type represented in our sample, one is Jewish; the other is a regional organization composed of Guadalajara residents from Vera Cruz. The Lebanese and North Americans also have clubs, as do other Mexican locality groups, but these do not appear in our sample.

Hobby clubs. Avocational clubs, whose members pursue a skilled or technical interest for pleasure, are apparently not numerous in Mexico. An amateur radio club is the only one which turned up in our interviews.

THE PARTICIPATION PATTERN

A glance at Table 2 establishes the first important point regarding the participation pattern: namely, that a large number of our informants and their spouses do not belong to any formally organized voluntary association.

The majority of men (58.4 per cent) and

TABLE 2. NUMBER OF MEMBERSHIPS PER PERSON, CLASSIFIED BY SEX

Number of Memberships	Men		Women	
	N	Per Cent	N	Per Cent
0	108	58.4	156	67.8
1	63	34.0	52	22.6
2	9	4.9	16	7.0
3	4	2.2	5	2.2
4	1	0.5	0	0.0
5	0	0.0	1	0.4
Total	185	100.0	230	100.0

an even larger percentage of women (67.8 per cent) lack affiliation. Of those persons who participate, most hold membership in only one association. Only five men and six women in a total of 415 individuals belong to three or more. Only one person reported membership in as many as five associations.

Table 2 suggests the importance of sex as a variable in participation; this fact is brought into sharper focus when membership in the various kinds of associations is analyzed by sex, as in Table 3.

The overwhelming proportion (81.7 per cent) of women's memberships are concentrated in church-affiliated associations. Society clubs, with less than 10 per cent of the total female memberships, are the only other organizations in which there occurs more than a nominal female representation. Male memberships, on the other hand, are more evenly distributed among the various types of associations. Their heaviest concentration is in athletic and sport organizations, which

TABLE 3. MEMBERSHIPS CLASSIFIED BY SEX AND TYPE OF ASSOCIATION

Type of Association	Memberships					
	Male		Female		Total	
	N	Per Cent	N	Per Cent	N	Per Cent
Church-affiliated	15	15.5	85	81.7	100	49.8
Athletic and sport	27	27.8	3	2.9	30	14.9
Labor union	23	23.7	3	2.9	26	12.9
Society	13	13.4	10	9.6	23	11.4
Professional and learned	5	5.2	0	0.0	5	2.5
International service	4	4.1	1	1.0	5	2.5
Mutualist	4	4.1	1	1.0	5	2.5
National-local business and professional	3	3.1	0	0.0	3	1.5
Ethnic and regional	2	2.1	1	1.0	3	1.5
Hobby	1	1.0	0	0.0	1	0.5
Total	97	100.0	104	100.0	201	100.0

TABLE 4. NUMBER OF MEMBERSHIPS PER PERSON BY INCOME

Memberships per Person	Household Income per Month, in Pesos					
	Under 250	250-500	500-1000	1000-2000	2000-3000	Over 3000
0	41	65	76	60	16	6
1	9	25	28	33	16	4
2	0	8	7	2	1	7
3	1	0	1	0	0	7
4	0	0	0	1	0	0
5	0	0	0	1	0	0
Total number of persons	51	98	112	97	33	24

account for some 28 per cent of the male total. The next highest percentages for men are found in labor unions, church-affiliated associations, and society clubs in that order. In brief, church-affiliated societies are composed largely of women; all other voluntary associations, with the single exception of society clubs, where the representation is roughly equal, tend to be pretty much a male domain.

Table 3 also provides the necessary data to evaluate the relative importance, from the standpoint of membership, of the various associational types according to our classification. Church-affiliated societies head the list with almost half of all the memberships represented in our sample.¹¹ Athletic and sport clubs are next most numerous, with labor unions in third place. These three types account for over three-fourths (77.6 per cent) of all affiliations. Society clubs, with 11.4 per cent of the total, hold fourth place in this particular sample. All other associational types have only nominal representation.

Studies in the United States have revealed socio-economic status as one of the most important variables in participation in voluntary associations: the higher the socio-economic status, the greater the participation. Table 4, which gives the number of memberships per person by our six income categories, and Table 5, which gives the ratio of memberships to persons within each income category, show the effect of socio-economic status upon participation in this Mexican city.

¹¹ Of the 100 memberships in this category, 45 are in the Catholic Action, 28 are devotional, and 27 are modern special-interest organizations.

It will be seen that the sample falls into three distinct groupings when classified by income as in Tables 4 and 5. The first grouping is composed of people in the lowest income category—those belonging to a household with incomes of 250 pesos a month or less. Four-fifths of this category (which constitutes a considerable but undetermined proportion of the general population) lack affiliation with any formal voluntary association whatsoever. Above this lowest stratum there is more participation, but until the income category of 2,000 to 3,000 pesos per month is reached less than half of the people participate. In the highest income category, however, 18 of the 24 persons included have memberships, 14 have two or more, and 7 of the 24 have as many as three memberships each.

TABLE 5. RATIO OF MEMBERSHIPS TO PERSONS BY INCOME

Income	Number of Mem- berships	Number of Persons	Ratio
Under 250	12	51	.24
250-500	41	98	.42
500-1000	45	112	.40
1000-2000	46	97	.47
2000-3000	18	33	.55
Over 3000	39	24	1.63

As might be expected, the relationship between participation and income varies according to the type of association. The distribution of memberships in the various types of associations by income categories is provided in Table 6.

Membership in church societies is spread rather evenly throughout the sample. There

is some participation in athletic associations even in the lowest income groups—which is mainly due to the fact that some business firms in Guadalajara pay the dues of employees who wish to participate in an athletic club. Membership in labor unions tends to be concentrated in the groups earning less than 1,000 pesos per month; the three exceptions to this rule among our informants belong to the "aristocracy of labor," the railway engineers and firemen. But other associations show a tendency to concentrate in the higher income categories. This is particularly noticeable in the case of society clubs—four-fifths of the memberships in this

(with the possible exception of the highest) less participation than would be expected for similar strata in the United States. Non-participation is particularly characteristic of low-income groups. But middle-income people are also typically without affiliations, although they participate to a greater extent than do low-income persons. It is only in the privileged category of persons belonging to a family unit enjoying a monthly income of 2,000 pesos or more that a majority may be expected to hold membership, and it is only in the strictly upper-class circles that membership in voluntary associations becomes the rule for most persons. There is,

TABLE 6. MEMBERSHIPS BY INCOME AND TYPE OF ASSOCIATION

Type of Association	Income											
	Under 250		250-500		500-1000		1000-2000		2000-3000		Over 3000	
	N	Per Cent	N	Per Cent	N	Per Cent	N	Per Cent	N	Per Cent	N	Per Cent
Church-affiliated	7	58.3	25	61.0	18	40.0	28	60.9	11	61.1	11	28.2
Athletic and sport	2	16.7	4	9.8	11	24.4	7	15.2	2	11.1	4	10.2
Labor union	3	25.0	11	26.8	9	20.0	1	2.2	2	11.1	0	0.0
Society	0	0.0	0	0.0	0	0.0	2	4.3	2	11.1	19	48.7
Professional and learned	0	0.0	0	0.0	1	2.2	4	8.7	0	0.0	0	0.0
International service	0	0.0	0	0.0	1	2.2	1	2.2	0	0.0	3	7.7
Mutualist	0	0.0	0	0.0	2	4.4	1	2.2	1	5.6	1	2.6
National-local business and professional	0	0.0	0	0.0	2	4.4	0	0.0	0	0.0	1	2.6
Ethnic and regional	0	0.0	1	2.4	0	0.0	2	4.3	0	0.0	0	0.0
Hobby	0	0.0	0	0.0	1	2.2	0	0.0	0	0.0	0	0.0
Total	12	100.0	41	100.0	45	100.0	46	100.0	18	100.0	39	100.0

kind of association are found among those persons enjoying a monthly income over 3,000 pesos.

CONCLUSIONS

The conclusions, substantive and theoretical, to be reached from an exploratory study must, of necessity, be stated tentatively. With this fact in mind, attention may be called to the following as some of the more significant empirical results and theoretical implications of this study.

1. While the percentage cannot be stated precisely from this sample, the data make it clear that a very large proportion of the population of Guadalajara lack affiliation with a formally organized voluntary association. There is, in general, a direct relationship between socio-economic status and participation, but there seems to be at all levels

nevertheless, some participation at all income levels.

2. The study shows that three types of associations—church-affiliated, athletic and sport, and labor unions—account for the great majority of organizational memberships. In a representative sample of the population, the relative importance of these associations as against all others would be even greater than it is in our sample, which is weighted in favor of the middle- and upper-income groups.

3. Sex differences emerge as one of the most striking features of the participation pattern. Less women participate than men, and women's memberships are overwhelmingly concentrated in church-affiliated associations. Society clubs are the only other type of association in which women are prominently represented.

4. If the types of associations represented in our sample are compared with similar classifications made for the United States, it will be seen that most of the associations found in the Mexican city have direct or close parallels in American cities.¹² This is not accidental—many Mexican associations are patterned after similar organizations in the United States, and others, notably the Catholic Action, originated in Europe. In other words, few modern Mexican voluntary associations represent an indigenous development but are the result (at least in form) of cultural diffusion. The appearance of athletic organizations, labor unions, mutualist insurance societies, the Catholic Action, service clubs, country clubs, and professional societies in Mexico seems to have followed rather closely the accelerated urban growth within the country during the last 50 years.¹³ But the extent to which this development of voluntary associations has resulted from the functional necessities of an urban social structure *per se* remains obscured. Would similar types of associations have appeared with urbanization if American and European models had not already existed? While purely hypothetical, this question brings to focus the fact that acculturation as well as urbanization must be taken into account in any theoretical interpretation of the role of voluntary associations in modern Mexican society. Probably this generalization has application beyond Mexico. Since urban communities everywhere are now stamped irrevocably with many cultural patterns developed originally in the European and North American centers of modern urbanism, it seems inevitable that acculturation will loom large as a complicating factor in cross-cultural urban studies.¹⁴

5. Nevertheless, in spite of the similarities

brought about by acculturation, significant differences appear when Mexican and American associational types are compared. The diffusion which has taken place has been selective. Many associations which are numerous in the United States are conspicuous by their absence in Guadalajara. We do not find (at least as represented by our sample) the secular women's clubs which occur in profusion among the middle and upper classes in the United States. Nor are there present the numerous fraternal lodges. There are no small, local political clubs in Guadalajara which correspond to the ward clubs of a large American city. Nor are there veterans' associations, in spite of the many ex-soldiers of the Revolution.

These differences are of course identifiable with divergencies in the cultural traditions and historical experiences of the two countries. The presence and absence of secular women's clubs reflect the American and Mexican definition of the social role of women and the structure of the family. Fraternal lodges, derivative in large part from the Masonic movement, are presumably more compatible with Protestantism than with Catholicism. Political clubs of the type found in the United States presuppose well-developed representative government. Veterans' organizations are also evidence of the American citizen's greater experience with pressure-blocs. These cultural differences remain and are important despite superficial similarities in the mode of life of urban Mexicans and that of Americans. Certainly they have affected the types of voluntary associations developed in Mexico and the degree of participation in them.

6. Current theory concerning the role of voluntary associations in social structure has mainly interpreted this phenomenon as a functional necessity of an urbanized society. The present study, while much too limited really to test this hypothesis, does indicate the likelihood that cities of similar size in radically different cultural areas may be expected to show wide variations in the number and type of voluntary associations, and amount of participation in them. The full theoretical implications of these variations, if they exist, must await the further development of comparative urban sociology.

¹² For example, Mirra Komarovsky, "The Voluntary Associations of Urban Dwellers," *American Sociological Review*, XI (December, 1946), pp. 686-698.

¹³ The author, in collaboration with Lillian Ota Dotson, is now studying the origins of voluntary associations in Mexico, using newspaper sources.

¹⁴ Beals discusses the problem of acculturation in a somewhat different context in his article previously cited, "Urbanism, Urbanization, and Acculturation."

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SOME PRINCIPLES OF STRATIFICATION: A CRITICAL ANALYSIS *

MELVIN M. TUMIN

Princeton University

THE fact of social inequality in human society is marked by its ubiquity and its antiquity. Every known society, past and present, distributes its scarce and demanded goods and services unequally. And there are attached to the positions which command unequal amounts of such goods and services certain highly morally-toned evaluations of their importance for the society.

The ubiquity and the antiquity of such inequality has given rise to the assumption that there must be something both inevitable and positively functional about such social arrangements.

Clearly, the truth or falsity of such an assumption is a strategic question for any general theory of social organization. It is therefore most curious that the basic premises and implications of the assumption have only been most casually explored by American sociologists.

The most systematic treatment is to be found in the well-known article by Kingsley Davis and Wilbert Moore, entitled "Some Principles of Stratification."¹ More than

twelve years have passed since its publication, and though it is one of the very few treatments of stratification on a high level of generalization, it is difficult to locate a single systematic analysis of its reasoning. It will be the principal concern of this paper to present the beginnings of such an analysis.

The central argument advanced by Davis and Moore can be stated in a number of sequential propositions, as follows:

- (1) Certain positions in any society are functionally more important than others, and require special skills for their performance.
- (2) Only a limited number of individuals in any society have the talents which can be trained into the skills appropriate to these positions.
- (3) The conversion of talents into skills involves a training period during which sacrifices of one kind or another are made by those undergoing the training.
- (4) In order to induce the talented persons to undergo these sacrifices and acquire the training, their future positions must carry an inducement value in the form of differential, i.e., privileged and disproportionate access to the scarce and desired rewards which the society has to offer.²
- (5) These scarce and desired goods consist of the rights and perquisites attached to,

integration asserted by Parsons is with the fact of the normative orientation of any society. Certain crucial lines of connection are left unexplained, however, in this article, and in the Davis and Moore article of 1945 only some of these lines are made explicit.

² The "scarcity and demand" qualities of goods and services are never explicitly mentioned by Davis and Moore. But it seems to the writer that the argument makes no sense unless the goods and services are so characterized. For if rewards are to function as differential inducements they must not only be differentially distributed but they must be both scarce and demanded as well. Neither the scarcity of an item by itself nor the fact of its being in demand is sufficient to allow it to function as a differential inducement in a system of unequal rewards. Leprosy is scarce and oxygen is highly demanded.

* The writer has had the benefit of a most helpful criticism of the main portions of this paper by Professor W. J. Goode of Columbia University. In addition, he has had the opportunity to expose this paper to criticism by the Staff Seminar of the Sociology Section at Princeton. In deference to a possible rejoinder by Professors Moore and Davis, the writer has not revised the paper to meet the criticisms which Moore has already offered personally.

¹ *American Sociological Review*, X (April, 1945), pp. 242-249. An earlier article by Kingsley Davis, entitled, "A Conceptual Analysis of Stratification," *American Sociological Review*, VII (June, 1942), pp. 309-321, is devoted primarily to setting forth a vocabulary for stratification analysis. A still earlier article by Talcott Parsons, "An Analytical Approach to the Theory of Social Stratification," *American Journal of Sociology*, XLV (November, 1940), pp. 849-862, approaches the problem in terms of why "differential ranking is considered a really fundamental phenomenon of social systems and what are the respects in which such ranking is important." The principal line of

or built into, the positions, and can be classified into those things which contribute to (a) sustenance and comfort, (b) humor and diversion, (c) self-respect and ego expansion.

- (6) This differential access to the basic rewards of the society has as a consequence the differentiation of the prestige and esteem which various strata acquire. This may be said, along with the rights and perquisites, to constitute institutionalized social inequality, i.e., stratification.
- (7) Therefore, social inequality among different strata in the amounts of scarce and desired goods, and the amounts of prestige and esteem which they receive, is both positively functional and inevitable in any society.

Let us take these propositions and examine them *seriatim*.³

(1) *Certain positions in any society are more functionally important than others and require special skills for their performance.*

The key term here is "functionally important." The functionalist theory of social organization is by no means clear and explicit about this term. The minimum common referent is to something known as the "survival value" of a social structure.⁴ This concept immediately involves a number of perplexing questions. Among these are: (a) the issue of minimum vs. maximum survival, and the possible empirical referents which can be given to those terms; (b) whether such a proposition is a useless tautology since any *status quo* at any given moment is nothing more and nothing less than everything present in the *status quo*. In these terms, all acts and structures must be judged positively functional in that they constitute essential portions of the *status quo*; (c) what kind of calculus of functionality exists which will enable us, at this point in our development, to add and subtract long and short range consequences, with their mixed qualities, and arrive at some summative

judgment regarding the rating an act or structure should receive on a scale of greater or lesser functionality? At best, we tend to make primarily intuitive judgments. Often enough, these judgments involve the use of value-laden criteria, or, at least, criteria which are chosen in preference to others not for any sociologically systematic reasons but by reason of certain implicit value preferences.

Thus, to judge that the engineers in a factory are functionally more important to the factory than the unskilled workmen involves a notion regarding the dispensability of the unskilled workmen, or their replaceability, relative to that of the engineers. But this is not a process of choice with infinite time dimensions. For at some point along the line one must face the problem of adequate motivation for *all* workers at all levels of skill in the factory. In the long run, *some* labor force of unskilled workmen is as important and as indispensable to the factory as *some* labor force of engineers. Often enough, the labor force situation is such that this fact is brought home sharply to the entrepreneur in the short run rather than in the long run.

Moreover, the judgment as to the relative indispensability and replaceability of a particular segment of skills in the population involves a prior judgment about the bargaining-power of that segment. But this power is itself a culturally shaped *consequence* of the existing system of rating, rather than something inevitable in the nature of social organization. At least the contrary of this has never been demonstrated, but only assumed.

A generalized theory of social stratification must recognize that the prevailing system of inducements and rewards is only one of many variants in the whole range of possible systems of motivation which, at least theoretically, are capable of working in human society. It is quite conceivable, of course, that a system of norms could be institutionalized in which the idea of threatened withdrawal of services, except under the most extreme circumstances, would be considered as absolute moral anathema. In such a case, the whole notion of relative functionality, as advanced by Davis and Moore, would have to be radically revised.

(2) *Only a limited number of individuals*

³ The arguments to be advanced here are condensed versions of a much longer analysis entitled, *An Essay on Social Stratification*. Perforce, all the reasoning necessary to support some of the contentions cannot be offered within the space limits of this article.

⁴ Davis and Moore are explicitly aware of the difficulties involved here and suggest two "independent clues" other than survival value. See footnote 3 on p. 244 of their article.

in any society have the talents which can be trained into the skills appropriate to these positions (i.e., the more functionally important positions).

The truth of this proposition depends at least in part on the truth of proposition 1 above. It is, therefore, subject to all the limitations indicated above. But for the moment, let us assume the validity of the first proposition and concentrate on the question of the rarity of appropriate talent.

If all that is meant is that in every society there is a *range* of talent, and that some members of any society are by nature more talented than others, no sensible contradiction can be offered, but a question must be raised here regarding the amount of sound knowledge present in any society concerning the presence of talent in the population.

For, in every society there is some demonstrable ignorance regarding the amount of talent present in the population. *And the more rigidly stratified a society is, the less chance does that society have of discovering any new facts about the talents of its members.* Smoothly working and stable systems of stratification, wherever found, tend to build-in obstacles to the further exploration of the range of available talent. This is especially true in those societies where the opportunity to discover talent in any one generation varies with the differential resources of the parent generation. Where, for instance, access to education depends upon the wealth of one's parents, and where wealth is differentially distributed, large segments of the population are likely to be deprived of the chance even to *discover* what are their talents.

Whether or not differential rewards and opportunities are functional in any one generation, it is clear that if those differentials are allowed to be socially inherited by the next generation, then, the stratification system is specifically dysfunctional for the discovery of talents in the next generation. In this fashion, systems of social stratification tend to limit the chances available to maximize the efficiency of discovery, recruitment and training of "functionally important talent."⁵

⁵ Davis and Moore state this point briefly on p. 248 but do not elaborate it.

Additionally, the unequal distribution of rewards in one generation tends to result in the unequal distribution of motivation in the succeeding generation. Since motivation to succeed is clearly an important element in the entire process of education, the unequal distribution of motivation tends to set limits on the possible extensions of the educational system, and hence, upon the efficient recruitment and training of the widest body of skills available in the population.⁶

Lastly, in this context, it may be asserted that there is some noticeable tendency for elites to restrict further access to their privileged positions, once they have sufficient power to enforce such restrictions. This is especially true in a culture where it is possible for an elite to contrive a high demand and a proportionately higher reward for its work by restricting the numbers of the elite available to do the work. The recruitment and training of doctors in modern United States is at least partly a case in point.

Here, then, are three ways, among others which could be cited, in which stratification systems, once operative, tend to reduce the survival value of a society by limiting the search, recruitment and training of functionally important personnel far more sharply than the facts of available talent would appear to justify. It is only when there is genuinely equal access to recruitment and training for all potentially talented persons that differential rewards can conceivably be justified as functional. And stratification systems are apparently *inherently antagonistic* to the development of such full equality of opportunity.

(3) *The conversion of talents into skills involves a training period during which sacrifices of one kind or another are made by those undergoing the training.*

Davis and Moore introduce here a concept, "sacrifice" which comes closer than any of the rest of their vocabulary of analysis to being a direct reflection of the ration-

⁶ In the United States, for instance, we are only now becoming aware of the amount of productivity we, as a society, lose by allocating inferior opportunities and rewards, and hence, inferior motivation, to our Negro population. The actual amount of loss is difficult to specify precisely. Some rough estimate can be made, however, on the assumption that there is present in the Negro population about the same range of talent that is found in the White population.

alizations, offered by the more fortunate members of a society, of the rightness of their occupancy of privileged positions. It is the least critically thought-out concept in the repertoire, and can also be shown to be least supported by the actual facts.

In our present society, for example, what are the sacrifices which talented persons undergo in the training period? The possibly serious losses involve the surrender of earning power and the cost of the training. The latter is generally borne by the parents of the talented youth undergoing training, and not by the trainees themselves. But this cost tends to be paid out of income which the parents were able to earn generally by virtue of *their* privileged positions in the hierarchy of stratification. That is to say, the parents' ability to pay for the training of their children is part of the differential *reward* they, the parents, received for their privileged positions in the society. And to charge this sum up against sacrifices made by the youth is falsely to perpetrate a bill or a debt already paid by the society to the parents.

So far as the sacrifice of earning power by the trainees themselves is concerned, the loss may be measured relative to what they might have earned had they gone into the labor market instead of into advanced training for the "important" skills. There are several ways to judge this. One way is to take all the average earnings of age peers who did go into the labor market for a period equal to the average length of the training period. The total income, so calculated, roughly equals an amount which the elite can, on the average, earn back in the first decade of professional work, over and above the earnings of his age peers who are not trained. Ten years is probably the maximum amount needed to equalize the differential.⁷ There remains, on the average, twenty years of work during each of which the skilled person then goes on to earn far more than his unskilled age peers. And, what is often forgotten, there is then still another ten or fifteen year period during which the skilled person continues to work and earn when his unskilled age peer is

either totally or partially out of the labor market by virtue of the attrition of his strength and capabilities.

One might say that the first ten years of differential pay is perhaps justified, in order to regain for the trained person what he lost during his training period. But it is difficult to imagine what would justify continuing such differential rewards beyond that period.

Another and probably sounder way to measure how much is lost during the training period is to compare the per capita income available to the trainee with the per capita income of the age peer on the untrained labor market during the so-called sacrificial period. If one takes into account the earlier marriage of untrained persons, and the earlier acquisition of family dependents, it is highly dubious that the per capita income of the wage worker is significantly larger than that of the trainee. Even assuming, for the moment, that there is a difference, the amount is by no means sufficient to justify a lifetime of continuing differentials.

What tends to be completely overlooked, in addition, are the psychic and spiritual rewards which are available to the elite trainees by comparison with their age peers in the labor force. There is, first, the much higher prestige enjoyed by the college student and the professional-school student as compared with persons in shops and offices. There is, second, the extremely highly valued privilege of having greater opportunity for self-development. There is, third, all the psychic gain involved in being allowed to delay the assumption of adult responsibilities such as earning a living and supporting a family. There is, fourth, the access to leisure and freedom of a kind not likely to be experienced by the persons already at work.

If these are never taken into account as rewards of the training period it is not because they are not concretely present, but because the emphasis in American concepts of reward is almost exclusively placed on the material returns of positions. The emphases on enjoyment, entertainment, ego enhancement, prestige and esteem are introduced only when the differentials in these which accrue to the skilled positions need to be justified. If these other rewards were

⁷ These are only very rough estimates, of course, and it is certain that there is considerable income variation within the so-called elite group, so that the proposition holds only relatively more or less.

taken into account, it would be much more difficult to demonstrate that the training period, as presently operative, is really sacrificial. Indeed, it might turn out to be the case that even at this point in their careers, the elite trainees were being differentially rewarded relative to their age peers in the labor force.

All of the foregoing concerns the quality of the training period under our present system of motivation and rewards. Whatever may turn out to be the factual case about the present system—and the factual case is moot—the more important theoretical question concerns the assumption that the training period under *any* system must be sacrificial.

There seem to be no good theoretical grounds for insisting on this assumption. For, while under any system certain costs will be involved in training persons for skilled positions, these costs could easily be assumed by the society-at-large. Under these circumstances, there would be no need to compensate anyone in terms of differential rewards once the skilled positions were staffed. In short, there would be no need or justification for stratifying social positions on these grounds.

(4) In order to induce the talented persons to undergo these sacrifices and acquire the training, their future positions must carry an inducement value in the form of differential, i.e., privileged and disproportionate access to the scarce and desired rewards which the society has to offer.

Let us assume, for the purposes of the discussion, that the training period is sacrificial and the talent is rare in every conceivable human society. There is still the basic problem as to whether the allocation of differential rewards in scarce and desired goods and services is the only or the most efficient way of recruiting the appropriate talent to these positions.

For there are a number of alternative motivational schemes whose efficiency and adequacy ought at least to be considered in this context. What can be said, for instance, on behalf of the motivation which De Man called "joy in work," Veblen termed "instinct for workmanship" and which we latterly have come to identify as "intrinsic work satisfaction?" Or, to what extent could the motivation of "social duty" be institu-

tionalized in such a fashion that self interest and social interest come closely to coincide? Or, how much prospective confidence can be placed in the possibilities of institutionalizing "social service" as a widespread motivation for seeking one's appropriate position and fulfilling it conscientiously?

Are not these types of motivations, we may ask, likely to prove most appropriate for precisely the "most functionally important positions?" Especially in a mass industrial society, where the vast majority of positions become standardized and routinized, it is the skilled jobs which are likely to retain most of the quality of "intrinsic job satisfaction" and be most readily identifiable as socially serviceable. Is it indeed impossible then to build these motivations into the socialization pattern to which we expose our talented youth?

To deny that such motivations could be institutionalized would be to overclaim our present knowledge. In part, also, such a claim would seem to deprive from an assumption that what has not been institutionalized yet in human affairs is incapable of institutionalization. Admittedly, historical experience affords us evidence we cannot afford to ignore. But such evidence cannot legitimately be used to deny absolutely the possibility of heretofore untried alternatives. Social innovation is as important a feature of human societies as social stability.

On the basis of these observations, it seems that Davis and Moore have stated the case much too strongly when they insist that a "functionally important position" which requires skills that are scarce, "must command great prestige, high salary, ample leisure, and the like," if the appropriate talents are to be attracted to the position. Here, clearly, the authors are postulating the unavoidability of very specific types of rewards and, by implication, denying the possibility of others.

(5) These scarce and desired goods consist of rights and perquisites attached to, or built into, the positions and can be classified into those things which contribute to (a) sustenance and comfort; (b) humor and diversion; (c) self respect and ego expansion.

(6) This differential access to the basic rewards of the society has as a consequence the differentiation of the prestige and esteem

which various strata acquire. This may be said, along with the rights and perquisites, to constitute institutionalized social inequality, i.e., stratification.

With the classification of the rewards offered by Davis and Moore there need be little argument. Some question must be raised, however, as to whether any reward system, built into a general stratification system, must allocate equal amounts of all three types of reward in order to function effectively, or whether one type of reward may be emphasized to the virtual neglect of others. This raises the further question regarding which type of emphasis is likely to prove most effective as a differential inducer. Nothing in the known facts about human motivation impels us to favor one type of reward over the other, or to insist that all three types of reward must be built into the positions in comparable amounts if the position is to have an inducement value.

It is well known, of course, that societies differ considerably in the kinds of rewards they emphasize in their efforts to maintain a reasonable balance between responsibility and reward. There are, for instance, numerous societies in which the conspicuous display of differential economic advantage is considered extremely bad taste. In short, our present knowledge commends to us the possibility of considerable plasticity in the way in which different types of rewards can be structured into a functioning society. This is to say, it cannot yet be demonstrated that it is *unavoidable* that differential prestige and esteem shall accrue to positions which command differential rewards in power and property.

What does seem to be unavoidable is that differential prestige shall be given to those in any society who conform to the normative order as against those who deviate from that order in a way judged immoral and detrimental. On the assumption that the continuity of a society depends on the continuity and stability of its normative order, some such distinction between conformists and deviants seems inescapable.

It also seems to be unavoidable that in any society, no matter how literate its tradition, the older, wiser and more experienced individuals who are charged with the en-

culturation and socialization of the young must have more power than the young, on the assumption that the task of effective socialization demands such differential power.

But this differentiation in prestige between the conformist and the deviant is by no means the same distinction as that between strata of individuals each of which operates *within* the normative order, and is composed of adults. The *latter* distinction, in the form of differentiated rewards and prestige between social strata is what Davis and Moore, and most sociologists, consider the structure of a stratification system. The *former* distinctions have nothing necessarily to do with the workings of such a system nor with the efficiency of motivation and recruitment of functionally important personnel.

Nor does the differentiation of power between young and old necessarily create differentially valued strata. For no society rates its young as less morally worthy than its older persons, no matter how much differential power the older ones may temporarily enjoy.

(7) *Therefore, social inequality among different strata in the amounts of scarce and desired goods, and the amounts of prestige and esteem which they receive, is both positively functional and inevitable in any society.*

If the objections which have heretofore been raised are taken as reasonable, then it may be stated that the only items which any society *must* distribute unequally are the power and property necessary for the performance of different tasks. If such differential power and property are viewed by all as commensurate with the differential responsibilities, and if they are culturally defined as *resources* and not as rewards, then, no differentials in prestige and esteem need follow.

Historically, the evidence seems to be that every time power and property are distributed unequally, no matter what the cultural definition, prestige and esteem differentiations have tended to result as well. Historically, however, no systematic effort has ever been made, under propitious circumstances, to develop the tradition that each man is as socially worthy as all other men so long as he performs his appropriate

tasks conscientiously. While such a tradition seems utterly utopian, no known facts in psychological or social science have yet demonstrated its impossibility or its dysfunctionality for the continuity of a society. The achievement of a full institutionalization of such a tradition seems far too remote to contemplate. Some successive approximations at such a tradition, however, are not out of the range of prospective social innovation.

What, then, of the "positive functionality" of social stratification? Are there other, negative, functions of institutionalized social inequality which can be identified, if only tentatively? Some such dysfunctions of stratification have already been suggested in the body of this paper. Along with others they may now be stated, in the form of provisional assertions, as follows:

(1) Social stratification systems function to limit the possibility of discovery of the full range of talent available in a society. This results from the fact of unequal access to appropriate motivation, channels of recruitment and centers of training.

(2) In foreshortening the range of available talent, social stratification systems function to set limits upon the possibility of expanding the productive resources of the society, at least relative to what might be the case under conditions of greater equality of opportunity.

(3) Social stratification systems function to provide the elite with the political power necessary to procure acceptance and dominance of an ideology which rationalizes the *status quo*, whatever it may be, as "logical," "natural" and "morally right." In this manner, social stratification systems function as essentially conservative influences in the societies in which they are found.

(4) Social stratification systems function to distribute favorable self-images unequally throughout a population. To the extent that such favorable self-images are requisite to the development of the creative potential inherent in men, to that extent stratification systems function to limit the development of this creative potential.

(5) To the extent that inequalities in social rewards cannot be made fully acceptable to the less privileged in a society, social stratification systems function to encourage hostility, suspicion and distrust among the various segments of a society and thus to limit the possibilities of extensive social integration.

(6) To the extent that the sense of significant membership in a society depends on one's place on the prestige ladder of the society, social stratification systems function to distribute unequally the sense of significant membership in the population.

(7) To the extent that loyalty to a society depends on a sense of significant membership in the society, social stratification systems function to distribute loyalty unequally in the population.

(8) To the extent that participation and apathy depend upon the sense of significant membership in the society, social stratification systems function to distribute the motivation to participate unequally in a population.

Each of the eight foregoing propositions contains implicit hypotheses regarding the consequences of unequal distribution of rewards in a society in accordance with some notion of the functional importance of various positions. These are empirical hypotheses, subject to test. They are offered here only as exemplary of the kinds of consequences of social stratification which are not often taken into account in dealing with the problem. They should also serve to reinforce the doubt that social inequality is a device which is uniformly functional for the role of guaranteeing that the most important tasks in a society will be performed conscientiously by the most competent persons.

The obviously mixed character of the functions of social inequality should come as no surprise to anyone. If sociology is sophisticated in any sense, it is certainly with regard to its awareness of the mixed nature of any social arrangement, when the observer takes into account long as well as short range consequences and latent as well as manifest dimensions.

SUMMARY

In this paper, an effort has been made to raise questions regarding the inevitability and positive functionality of stratification, or institutionalized social inequality in rewards, allocated in accordance with some notion of the greater and lesser functional importance of various positions. The possible alternative meanings of the concept "functional importance" has been shown to be one difficulty. The question of the

scarcity or abundance of available talent has been indicated as a principal source of possible variation. The extent to which the period of training for skilled positions may reasonably be viewed as sacrificial has been called into question. The possibility has been suggested that very different types of motivational schemes might conceivably be made to function. The separability of differentials in power and property considered as resources appropriate to a task from such differentials considered as rewards for the performance of a task has also been suggested. It has also been maintained that differentials in prestige and esteem do not

necessarily follow upon differentials in power and property when the latter are considered as appropriate resources rather than rewards. Finally, some negative functions, or dysfunctions, of institutionalized social inequality have been tentatively identified, revealing the mixed character of the outcome of social stratification, and casting doubt on the contention that

Social inequality is thus an unconsciously evolved device by which societies insure that the most important positions are conscientiously filled by the most qualified persons.⁸

⁸ Davis and Moore, *op. cit.*, p. 243.

REPLY

KINGSLEY DAVIS

Columbia University

Tumin's critique, almost as long as the article it criticizes, is unfortunately intended not to supplement or amend the Davis-Moore theory but to prove it wrong. The critique also sets a bad example from the standpoint of methodology. Nevertheless, it does afford us a meager opportunity to clarify and extend the original discussion. The latter, limited to eight pages, was so brief a treatment of so big a subject that it had to ignore certain relevant topics and telescope others. In the process of answering Tumin, a partial emendation can now be made.

GENERAL CONSIDERATIONS

Our critic seems to labor under four major difficulties, two of a methodological and two of a substantive character. First, he appears not so much interested in understanding institutionalized inequality as in getting rid of it. By insinuating that we are "justifying" such inequality, he falls into the usual error of regarding a causal explanation of something as a justification of it. He himself offers no explanation for the universality of stratified inequality. He argues throughout his critique that stratification does not have to be, instead of trying to understand why it is. Our interest, however, was only in the latter question. If Tumin had chosen to state our propositions in our own words rather than his, he could not have pictured us as concerned with the question of whether stratification is "avoidable."

Second, Tumin confuses abstract, or theoretical, reasoning on the one hand with raw empirical generalizations on the other. Much of his critique accordingly rests on the fallacy

of misplaced concreteness. Our article dealt with stratified inequality as a general property of social systems. It represented a high degree of abstraction, because there are obviously other aspects of society which in actuality affect the operation of the prestige element. It is therefore impossible to move directly from the kind of propositions we were making to descriptive propositions about, say, American society.

Third, in concentrating on only one journal article, Tumin has ignored other theoretical contributions by the authors on stratification and on other relevant aspects of society. He has thus both misrepresented the theory and raised questions that were answered elsewhere.

Fourth, by ignoring additions to the theory in other places, Tumin has failed to achieve consistency in his use of the concept "stratification." The first requirement, in this connection, is to distinguish between stratified and non-stratified statuses. One of the authors under attack has shown the difference to hinge on the family. "Those positions that may be combined in the same legitimate family—viz., positions based on sex, age, and kinship—do not form part of the system of stratification. On the other hand those positions that are socially prohibited from being combined in the same legal family—viz., different caste or class positions—constitute what we call stratification."¹ This distinction is basic, but in addition it is necessary to realize that two different questions can be asked about stratified positions: (a) Why are different evaluations and rewards given

¹ Kingsley Davis, *Human Society*, New York: Macmillan, 1949, p. 364.

to the different *positions*? (b) How do *individuals* come to be distributed in these positions? Our theory was designed to answer the first question by means of the second. But much confusion results, as illustrated by Tumin's ambiguities, if the term "stratification" is used in such a way as to overlook the distinction between the two.

THE SPECIFIC CRITICISMS

It will be seen that these four difficulties plague Tumin throughout his remarks and lead to much obfuscation. In answering his criticisms, we shall follow his sequence in terms of the propositions attributed to us.

1. *Differential Functional Importance of Positions.*

Tumin criticizes the idea of unequal functional importance on the grounds that the concept is unclear, unmeasurable, and evaluative, and that other systems of motivation are conceivable. The latter point is irrelevant, since the proposition in question says nothing whatever about motivation. So is the remark about "value-laden criteria," since no such criteria are advanced by us. As for the difficulty of measuring functional importance, we stated this before Tumin did, but he does not elect to discuss the two criteria suggested in our article. The difficulty of exact empirical measurement does not itself make a concept worthless; if so we should have to throw away virtually all theoretical concepts. Rough measures of functional importance are in fact applied in practice. In wartime, for example, decisions are made as to which industries and occupations will have priority in capital equipment, labor recruitment, raw materials, etc. In totalitarian countries the same is done in peacetime, as also in underdeveloped areas attempting to maximize their social and economic modernization. Individual firms must constantly decide which positions are essential and which not. There is nothing mystical about functional importance.

Tumin points out that the unskilled workmen in a factory are as important as the engineers. This is of course true, but we have maintained that the rating of positions is not a result of functional importance alone but also of the scarcity of qualified personnel. Any concrete situation is a product of both. It requires more capital to train an engineer than to train an unskilled worker, and so engineers would not be trained at all unless their work were considered important.

Actually Tumin does not deny the differential functional importance of positions. He disguises his agreement with the proposition by tendentious argumentation.

2. *The Strangulation of Talent by "Stratification."*

Tumin's objection to the idea of a scarcity of trained and talented personnel rests on the argument (a) that societies do not have a "sound knowledge" of talents in their populations and (b) that stratification interferes with, rather than facilitates, the selection of talented people. The first point is inconsequential, because a selective system—e.g., organized baseball—does not require a pre-existing knowledge of talent to be effective. The second point is crucial, but Tumin strangely fails to refer to a later treatment of this very problem by Davis.² In introducing the problem, Davis says: "One may object to the foregoing explanation of stratification [as contained in the Davis-Moore article] on the ground that it fits a competitive order but does not fit a non-competitive one. For instance, in a caste system it seems that people do not get their positions because of talent or training but rather because of birth. This criticism raises a crucial problem and forces an addition to the theory." The addition takes the following form: The theory in question is a theory explaining the differential prestige of *positions* rather than individuals. Even though a high-caste person occupies his rank because of his parents, this fact does not explain the high evaluation of the caste's *position* in the community. The low estate of sweeper as compared with priestly castes cannot be explained by saying that the sons of sweepers become sweepers and the sons of Brahmins become Brahmins. The explanation of the differential evaluation of strata must be sought elsewhere, in the survival value of drawing qualified people into the functionally most important positions. But since this is not the only functional necessity characterizing social systems, it is in actuality limited by certain other structures and requirements. Among the latter is the family, which limits vertical mobility by the mechanism of inheritance and succession. The family's limiting role, however, is never complete, for there is some vertical mobility in any society. Thus the selective effect of the prestige system exists in its pure form only abstractly, not concretely; and the same is true of the inheritance of status. Consequently, to say that in a given society there is partial inheritance of high positions is not to deny that at the same time the prestige system is operating to draw capable people into these positions.

One source of confusion in this argument is the ambiguity, mentioned above, of the term "stratification." On the one hand it is used by us to designate the institutionalized inequality

² *Human Society*, pp. 369-370.

of rewards as between broad strata.³ On the other hand, it is used (as Tumin does implicitly) to mean the inheritance of class status. With the latter definition, of course, the idea that stratification contributes to upward mobility is incongruous. One cannot expect a theory designed to account for the universal existence of institutionalized inequality as between positions to be, at the same time, an explanation of the inheritance of class status. However, it is possible to extend the theory by combining with it a general analysis of the family's articulation with the differential reward system in society. This has been done by Davis, and the result is that one can understand the combined existence in the same society of (a) a differential ranking of stratified positions, (b) a certain amount of vertical mobility, and (c) a certain amount of inheritance of status. It is this extension of the theory that Tumin ignores.

3. The "No Sacrifice" Criticism.

Tumin contends, in effect, that no differential rewards are necessary to induce individuals to qualify for functionally important positions, because they make no "sacrifices." In support of his view he says (a) that the family often makes the sacrifice for the offspring, (b) that the loss of earning power during the training period is negligible, and (c) that the prestige during the training period is high.

But point (a) confirms rather than denies the theory. It makes no essential difference that the family assumes some of the burden of training; the fact is that there is a burden. The differential ability of families to make such sacrifices of course comes back to the role of the family in limiting competition for status, which has already been discussed. The claim under point (b) that the loss of earning power during training is more than made up in the years following again confirms the theory, for we have said there is a differential reward for those attaining functionally important positions. As for point (c), the fact that the trainee may enjoy a standard of living higher than that of his already working age peers comes back to the family's status, already discussed. Nor does the claim that the psychic rewards are high during training offer any objection to the theory, because these psychic rewards are mainly a reflection of the anticipated rewards of an ultimately high status to be attained through training. It is amusing that throughout his discussion of "sacrifice" Tumin, though

thinking in terms of professional training, never once mentions the onerous necessity of studying. It is unfortunately true that most individuals regard hard study as burdensome, and it is something that the family cannot do for them. Many youths are unwilling to make this sacrifice and also many are incapable of doing it well enough to succeed. There are, however, many other kinds of hurdles to high position that would discourage an individual if it were not for the rewards offered. So difficult is it to get enough qualified personnel in good positions that the modern state undertakes to bear some of the costs, but it cannot bear them all.

4. Alternative Motivational Schemes.

Our critic contends that there are "a number of alternative motivational schemes" as efficient as differential rewards in motivating people to strive for important positions. Actually he mentions three: joy in work, sense of social service, and self interest. The third is obviously a ringer. Concerning it he asks, "To what extent could the motivation of 'social duty' be institutionalized in such a fashion that self interest and social interest come closely to coincide?" The answer is that such coinciding is not only possible but is actually accomplished—and our theory of social inequality explains how. This leaves, then, only two alternatives that offer any possible criticism of our views. In the eight-page article under attack, it was mentioned that one consideration is the unequal pleasantness of activities required by different positions, but space did not allow us to follow out the implications of this fact. The truth is that if everybody elected to do just what he wanted to do, the whole population would wind up in only a few types of position. A society could not operate on this basis, because it requires performance of a wide range of tasks. Surviving societies therefore evolve some system of inducements over and above the joy of work which motivates people to do what they would otherwise not do. Finally, as for the sense of social service, any sociologist should know the inadequacy of unrewarded altruism as a means of eliciting socially adequate behavior. It must be remembered that the differential rewards characterizing the status scale are not all material; they also lie in the good opinion and expectations of others and in the feeling of self-satisfaction at having stood well in others' eyes. No one will deny that joy of work and a sense of social service are actual motives, any more than one will deny the reality of the desire for esteem (in contrast to prestige) but in any society they are supplementary rather than alternative to the positional reward mechanism.

³ K. Davis, "A Conceptual Analysis of Stratification," *American Sociological Review*, 7 (June, 1942), pp. 309-321.

5-6. *Types of Rewards.*

The Davis-Moore article mentioned a rough tripartite classification of types of rewards occurring in stratified positions. Tumin says these may be unequally employed, that one society may emphasize one type more than another. This is true; we said nothing to the contrary. Tumin goes on to say that societies give approval to behavior that conforms with norms. This we certainly never disputed; indeed, in connection with positions, Davis has given a name to it—*esteem*, the kind of approval that comes with the faithful fulfillment of the duties of a position.⁴ The approval that comes with *having* a position, i.e., approval attached to the position and not to the degree of faithfulness in performing its duties, is called *prestige*. Whatever the words used, the distinction is important, but Tumin has confused the two. A social system, though it certainly utilizes esteem, is not entirely built on it, because there must be motivation not only to conform to the requirements of positions held but also to strive to get into positions. *Esteem* alone tends to produce a static society, *prestige* a mobile one. Tumin's statement that the position of the parent vis-a-vis the child is not part of the stratified system is true, but it agrees perfectly with Davis' distinction between stratified and non-stratified statuses, already mentioned as an essential part of the theory overlooked by Tumin.

7. *Inevitability and Disfunctionality.*

As the grand climax of his restatement of our views, Tumin has us concluding that social inequality is *inevitable* in society. Let it be repeated, we were not concerned with the indefinite or utopian future but with societies as we find them. No proof or disproof of a proposition about inevitability is possible. As "evidence" of his view of inevitability, Tumin *hopes* to see

⁴ "Conceptual Analysis of Stratification," *loc. cit.*, pp. 312-313; *Human Society*, pp. 93-94.

a society based on "the tradition that each man is as socially worthy as all other men so long as he performs his appropriate tasks conscientiously." But this is, once again, the idea of a society based exclusively on esteem. The question would still remain, how do people in the first place get distributed in their different positions with their "appropriate tasks?" One can hardly criticize a theory by ignoring the problem with which it deals.

Tumin goes on to point out ways in which stratification is disfunctional. In most of what he says, however, "stratification" is being used in the sense of inheritance of status. In so far as his assertion of disfunctionality is true, then, the culprit is the family, not the differential positional rewards. He also mentions unfavorable self-images, but the disfunctionality of these is not clear, because an unfavorable self-image may be a powerful stimulus to competitive and creative activity. The same comment can be made about the alleged disfunctionality of class conflict. Incidentally, in this part of his critique Tumin makes pronouncements of functionality with firm confidence, although in the early part he doubted the functionality could be determined.

The truth is that any aspect of society is functional in some ways and disfunctional in others. Our theory was designed to suggest some of the ways in which institutionalized positional inequality contributes to societies as going concerns. Otherwise it seems difficult if not impossible to explain the universal appearance of such inequality. Excrescencies and distortions certainly appear, but they do not completely negate the principle. Tumin's analysis of the disfunctions is unsophisticated because of his confusion as to what it is that *has* the disfunctions, because of his uncritical concept of function, and because of his lack of any clear notion of a social system as an equilibrium of forces of which the stratified positional scale is only one.

COMMENT

WILBERT E. MOORE

Princeton University

I generally concur with Professor Davis's reply, which is somewhat more comprehensive than the comments I had prepared independently. However, I should like to emphasize that there is no reason to deny to Professor Tumin the right and even the propriety of a theoretical approach to an equalitarian system, as long as relevant principles of social structure are somehow taken into account. I do not believe Professor Tumin has met the latter qualification.

With regard to the relevance of his criticism of our paper, I suggest that Professor Tumin made the major mistake of not explicitly defining social stratification, which in turn led him to assume that differential rewards and inequality of opportunity are the same thing. Neither theory nor evidence will support this equation, and making it true by implicit definition can only stand in the way of theoretically significant research.

SOCIAL CLASS IDENTIFICATION IN THE URBAN COMMUNITY *

NEAL GROSS

Harvard University

ONE approach to the study of social classes is to view them as referent groups. In contrast to a stratum which consists of a number of individuals occupying a relatively similar position on some objective characteristic such as income or occupation, a class is viewed as a "psychological phenomenon in the fullest sense of the word. That is, a man's class is a part of his ego, *a feeling on his part of belongingness to something*; and identification with something larger than himself."¹ Richard Centers' work constitutes the most developed treatment of the psychological approach to social classes. His research was based on a conceptual scheme termed "the interest theory of social class structure," in which a person's status and role as they are related to the economic aspects of social life are presumed to impose upon him values and attitudes which, to a high degree, are shared by others, and which give rise to a consciousness of membership in a particular social class.²

Centers investigated the interest theory formulation of social classes by asking two national quota control samples of male adults to place themselves in one of four social classes. Each respondent was asked the following question, "If you were asked to use

one of these four names for your social class which would you say you belonged in: the middle class, lower class, working class or upper class?"³

The cross section was also asked to select from a predetermined list of occupations, those which belonged in the class with which the respondent identified in addition to answering a series of attitude and other questions which were later cross classified with class self placement. Table 1 shows the distribution of responses Centers found when two quota control nation-wide samples were interviewed in 1945 and 1946.

TABLE 1. CLASS IDENTIFICATIONS OF A NATIONAL CROSS SECTION OF WHITE MALES AS REPORTED BY CENTERS *

Per Cent Saying	(July 1945) (N=1097)	(Feb. 1946) (N=1337)
Upper class	3	4
Middle class	43	36
Working class	51	52
Lower class	1	5
Don't know	1	3
Don't believe in classes	1	..

* A combination of Tables 18 and 19 reported in Richard Centers, *op. cit.*, p. 77.

Centers concluded from these findings: "The answers will convincingly dispel any doubt that Americans are class conscious, and quite as quickly quell any glib assertion like *Fortune's* 'America is Middle Class.'"⁴ He emphasizes that only an insignificant minority of his samples admit of no membership in some class; that a majority of the respondents claim to belong to the working class; and that whereas previous studies usually showed 80 to 90 per cent of the respondents claiming middle class membership, "only 43 per cent now say they are middle class."⁵

* This study was made possible by a research grant from The Louis W. and Maud Hill Foundation and through the help of interviewers recruited from the writer's course in Methods of Social Research at the University of Minnesota when he was a faculty member of that institution. The students in the Seminar in Methodological Problems of Social Research were of invaluable assistance in the processing and in the preliminary analysis of the data. The writer is especially indebted to A. O. Haller, George Helling, Pandora Gross and Alexander McEachern for their help in the project and their valuable suggestions and their criticisms of the central ideas of the paper.

¹ Richard Centers, *The Psychology of Social Classes*, Princeton, New Jersey: Princeton University Press, 1949, p. 27.

² *Ibid.*, p. 29.

³ *Ibid.*, p. 76.

⁴ *Ibid.*, p. 76.

⁵ *Ibid.*, p. 77.

These findings⁶ deserve careful scrutiny. They are based on the responses to a question in which the respondents are asked to place themselves in one of four named classes. If class consciousness is a part of a man's ego, a "feeling on his part of belongingness to something,"⁷ one would anticipate that, as a minimum condition of class identification with consequences for the individual's behavior and the social structure, people could verbalize such self identification without the aid of a set of alternative cue categories suggested by the interviewer.

It would be especially interesting to know the extent of differentiation in the kinds and the distributions of responses if the same population were asked to answer an open-ended question regarding their class identification as well as questions in which the respondent was asked to select one of a set of predetermined class names.

This paper reports the findings of such an inquiry, a part of a larger study on the social structure of the urban community.

METHODOLOGY

A major focus of the study was the determination of the extent of discrepancy in responses when respondents were given both an open-ended question and questions with a predetermined set of class categories in which the respondent was asked to indicate in which social class he belongs.

The research design required that especial attention be directed to the wording and placement of the social class questions in the schedule. After considerable prior experimentation, and as a result of the pretest, the following questions were utilized. To tap the respondent's social class self placement in an unstructured manner a sequence of open-ended questions was utilized: "There has been a lot of talk recently about social classes in the U. S. I wonder what you think about this. What social classes do you think there are in Minneapolis?" In addition, the respondent was asked to indicate in what ways families in the classes mentioned by

the respondent were different. After the respondent had been given an opportunity to talk freely about his perceptions of social classes, the strategic question for the purposes of this paper was asked: "Which one of these social classes are you in?" (Question I).

This sequence of questions gave the respondent a maximum opportunity to consider the topic prior to asking him to identify himself with a particular social class. If the respondent had been asked "cold" what social class he belongs to, a pretest showed that a large proportion of respondents would question the interviewer as to the meaning of social class. Since the respondent's perception unbiased by the remarks of the interviewer were desired, it was decided to use this more elaborate series of questions. Such a procedure maximized the possibility of getting an unbiased response to compare with the respondent's reply to the closed questions. It also provided data for an analysis of perceptions of the Minneapolis "social class system," a study to be reported in a later paper.

The open-ended question was asked, early in the schedule, after a preliminary series of questions concerning the attitudes of the respondents to such matters as rent control and the probability of war with Russia in the next five years. These preliminary questions were used to get the respondent to "open up" and to improve the rapport situation. Prior to asking the open-ended question the phrase "social classes" was not used in the schedule.

Much later in the schedule the closed question with Centers' predetermined set of class categories was asked. Since the respondent had earlier been given an opportunity to indicate his class identification in the open-ended question it was necessary to ask the Centers' type of question in such a way as to minimize the bias of having asked the earlier class question. To accomplish this, the question was asked in the following manner: "Some authorities claim that there are four social classes: middle class, lower class, working class and upper class. To which of these social classes would you say you belonged?" (Question II).

The use of the introductory phrase, "Some authorities claim. . . ." worked in the sense that few respondents referred

⁶ See Llewellyn Gross, "The Use of Class Concepts in Sociological Research," *American Journal of Sociology*, LIV (March, 1949), pp. 409-521 for an epistemological critique of Centers' techniques and conceptualization.

⁷ Richard Centers, *op. cit.*, p. 27.

back to their earlier response to the open-ended question and ninety per cent of the interviewees responded in the categories reported by Centers. It should be noted that the four class names were presented in the same order as asked by Centers in his nation-wide studies.

Still later in the schedule, another closed question with three predetermined class categories was asked the respondent. The wording used was, "Some people say there are three social classes in Minneapolis. They call them Lower, Middle and Upper Social Classes. Which would you put yourself in?" (Question III).

Another relevant methodological consideration is the population to which the findings apply. They do not apply to the population of all heads of families in Minneapolis. What was desired for the purpose of the inquiry was to determine the degree of consensus in responses to open-ended and closed "social class questions" from heads of families in sectors of the population differing widely on some "objective status" criterion. The central problem was to maximize the available time, personal and financial resources to achieve the "status" spread.

The most readily available and satisfactory "objective status" measure for Minneapolis was median or mean rental value of homes obtainable from the Census Tract Statistics of the U. S. Census.⁸ Each census tract is further divided into blocks, for which there is only a mean monthly rent listed.

The study design required that four clearly differentiated "objective status" geographical areas be selected and spaced on the rental scale so that differences between areas would be magnified. In order to secure such differences, all 121 census tracts in Minneapolis were ranked in order of magnitude of median monthly rent. The ranking was then divided into quartiles, and the

medians of the second and third quartiles chosen as the upper-middle and lower-middle rental tracts. To secure the maximum amount of rental spread, the highest and lowest rental tracts were picked to complete the foursome. They were as follows: tract 114 with 67.17 dollars per month rent; tract 76 with 35.89 dollars rent; tract 19 with 28.85 dollars rent; and tract 47 with 12.81 dollars rent. The appropriateness of the census tract selection based on 1940 data is supported by the 1950 census tract housing statistics.⁹

Personnel and financial limitations further necessitated the restriction of a maximum of 250 interviews in each tract. Contiguous blocks were chosen on the basis of mean monthly rent¹⁰ so that each tract was reduced to an area of from 10 to 18 blocks depending on the density of dwelling units. In tract 114 contiguous blocks with the *highest* mean rentals were picked until the 250 dwelling unit mark was reached. Conversely, in tract 47, blocks were similarly chosen with the *lowest* mean rents. These were designated as A and D areas respectively. In tracts 76 and 19 the blocks were chosen which most nearly approached the median for the tracts. These areas were designated as B and C respectively. As a result, area A represents the top of the high rentals, area B is the middle of the upper-middle quartile, area C is the middle of the lower-middle quartile, and area D is the bottom of the low rentals.¹¹ In short, the respondents do not represent all of the household heads in Minneapolis; rather they constitute household heads who fall into four widely spaced class intervals

⁹ The four census tracts in 1950 are clearly heterogeneous on median monthly rent. The 1950 mean contract monthly rent for census tracts 114, 76, 19 and 47 are respectively 79.88 dollars, 46.77 dollars, 36.86 dollars and 21.37 dollars. Source: U. S. Bureau of the Census, *U. S. Census of Population: 1950 Volume III, Census Tract Statistics*, Ch. 33. U. S. Govt. Printing Office, Washington, D. C., 1952, pp. 8, 10, 11 and 13.

¹⁰ Median rents were not available by blocks.

¹¹ This of course was the case in 1940 as reported above. In area D it will be noted in the later tabular analysis that the number of cases is 197, whereas in the other areas it approaches 250. This is explained by a clearing of certain slum homes during the 1940 decade, hence a smaller population in the area.

⁸ Since the data were collected in the last two months of 1950 the Census Tract Statistics for Minneapolis of the U. S. Census for 1950 were not available when the respondent selection procedure was developed. 1940 Census Tract Statistics were therefore used. A later comparison of the two sets of Census Tract Statistics show only slight differences in census tract ranking.

SOCIAL CLASS IDENTIFICATION IN THE URBAN COMMUNITY 401

TABLE 2. SELF CLASS PLACEMENT OF RESPONDENTS IN FOUR RESIDENTIAL AREAS IN MINNEAPOLIS IN RESPONSE TO CLOSED QUESTION III WITH THREE ALTERNATIVE CLASS NAMES *

Per Cent Saying	Area A (N=249)	Area B (N=245)	Area C (N=244)	Area D (N=197)	Total All Areas (N=935)
Upper class	7	4	2	6	5
Middle class	84	85	85	42	76
Lower class	..	5	6	36	10
No classes	3	1	2	2	2
Other class answers	3	3	2	4	3
Don't know	3	2	3	10	4

* Question III was: "Some people say there are three social classes in Minneapolis. They call them lower, middle and upper social classes. Which would you put yourself in?"

on the continuum of monthly rent in the city of Minneapolis. Hence, we have four separate populations which will be treated as independent populations in the analysis.

THE FINDINGS

The kinds and distributions of responses in the four rental areas in reply to Questions III, II and I are reported in Tables 2, 3, and 4 respectively.

(1) Using the U-M-L closed type question (Question III) the investigator would conclude that the majority of all respondents identify themselves with the middle class (Table 2). Over three-fourths of all respondents placed themselves in this category. Among the four city areas only in area D do we find less than a majority of the respondents so classifying themselves, although even here, we find the greatest proportion of the responses in this category. Such a response distribution would appear to support the findings of those investigators who claim that the majority

of American people identify themselves with the middle class.¹²

(2) Using the U-M-W-L closed type question (Question II) we find that the greatest proportion of all the respondents identify themselves with the working class (Table 3). The only exception to this finding among the four residential areas is in Area A, where the great majority of respondents identified themselves with the middle class. This finding would tend to support the contentions of Centers "that a majority of the respondents claim to belong to the working class and that whereas previous studies have usually shown 80 to 90 per cent of the respondents to claim middle class membership, only 43 per cent now say they are middle class."¹³

(3) Using the open-ended question (Question I) which offered no class designations to

¹² For example, Hadley Cantril, "Identification with Social and Economic Class," *Journal of Abnormal and Social Psychology*, 38 (1944), pp. 74-80.

¹³ *Ibid.*, p. 77.

TABLE 3. SELF CLASS PLACEMENT OF RESPONDENTS IN FOUR RESIDENTIAL AREAS IN MINNEAPOLIS IN RESPONSE TO CLOSED QUESTION II WITH FOUR ALTERNATIVE CLASS NAMES *

Per Cent Saying	Area A (N=249)	Area B (N=245)	Area C (N=244)	Area D (N=197)	Total All Areas (N=935)
Upper class	3	2	..	2	2
Middle class	77	34	42	9	42
Working class	8	57	51	72	45
Lower class	..	1	2	7	3
No classes	2	1	1	..	1
Other class answers	8	3	3	5	5
Don't know	2	2	1	5	2

* Question II was: "Some authorities claim that there are four social classes, middle class, lower class, working class and upper class. To which of these social classes would you say you belonged?"

TABLE 4. RESPONDENT CLASS DESIGNATION IN FOUR RESIDENTIAL AREAS IN MINNEAPOLIS IN RESPONSE TO AN OPEN-ENDED QUESTION (QUESTION I) * ON SELF CLASS PLACEMENT

Class Designation	Area A (N=249)	Area B (N=245)	Area C (N=244)	Area D (N=197)	Total All Areas (N=935)
Upper class	2	1	1
Middle class ^a	45	29	37	10	31
Working class ^b	1	13	15	15	11
Lower class	..	2	2	9	3
No classes ^c	19	13	16	6	14
Don't know	15	22	16	28	20
Other class responses ^d	16	12	12	22	15
No response ^e	2	9	2	9	5

* Question I was: "Which one of these social classes are you in?" after respondent had replied to the question "What social classes do you think there are in Minneapolis?"

^a Middle class includes any response for which the word "middle" was used. The ambiguity involved in the term "middle" will be treated in another paper on the perceptions of the class system.

^b Includes laboring class responses.

^c Includes responses "Don't think there are social classes" and "I don't belong to any social class."

^d Most frequent other class responses were: Poor class (three per cent), white class (one per cent), employer class (one per cent), white collar class (one per cent) and common class (one per cent).

^e Refers to those respondents who could not or would not reply after question had been repeated one or more times. The interviewers were not allowed to interpret the question to the respondent.

the respondent, however, provides another quite disparate set of findings (Table 4). For all respondents, only eleven per cent now claim working class membership; less than one-third (thirty-one per cent) claim middle class membership;¹⁴ one-fifth claim they do not know what social class they are in; one-seventh claim that they do not think there are any social classes in Minneapolis or disclaim class membership; another one-seventh (fifteen per cent) identify themselves with a wide range of social classes ranging from the "employer class" to the "common" or "poor class."

The impact of differential question wording on a respondent's self identification is

¹⁴ See footnote under Table 4.

placed in sharp focus in Table 5, where the class designations resulting from responses to the three questions for all rental areas combined is presented. Whereas Question III results in over three-fourths (seventy-six per cent) of the respondents "identifying" with the middle class, Question II results in a forty-two per cent middle class "identification" and the open-ended question (Question I) results in less than a one-third (thirty-one per cent) "identification" with this class. Question II yields a forty-five per cent "identification" with the working class as compared to only eleven per cent when the open-ended question is asked. Further, when Question I was asked, over one-third of the respondent

TABLE 5. A COMPARISON OF CLASS IDENTIFICATIONS RESULTING FROM RESPONSES TO QUESTIONS I, II AND III FOR THE COMBINED FOUR RENTAL AREAS *

Class Designations	Question I (open-ended)	Question II (U-M-W-L)	Question III (U-M-L)
Upper class	1	2	5
Middle class	31	42	76
Working class	11	45	..
Lower class	3	3	10
No classes	14	1	2
Don't know	20	2	4
Other class responses	15	5	3
No response	5

* The number of cases in each column is 935.

ents replied that they did not know what class they were in, that there were no social classes or that they did not belong to any social class.

In short, the conclusions the investigator emerges with, using the U-M-L or the U-M-W-L forced choice questions, are at great variance with the conclusions that emerge from the use of an open-ended class identification question.

These findings may be viewed as a documentation of the obvious but frequently forgotten proposition that all scientific data and research findings are artifacts of the techniques employed by the investigator. It has been demonstrated that different class identification findings pertaining to the same population emerge from the use of different cues offered respondents in the interview situation.

This conclusion requires consideration of the following question: "Which of the three question-wording techniques is most appropriate¹⁵ to elicit from respondents their social class identification when 'a man's class is a part of his ego, a feeling on his part of belongingness to something: and identification with something larger than himself?'"¹⁶ It is contended here that the open-ended methodology is more appropriate than the closed categories approach.

First, in tapping the dimension, "class identification," the research operations should provide the maximum opportunity for the respondent to express his lack of identification as well as his identification with a social class. Presenting the respondent with three or four alternatives and asking him to select the class to which he belongs minimizes the possibility that those respondents who have no class identification perceptions will report their actual perceptions. Offering the respondent no class categories tends to maximize this condition. Second, the research operation should provide minimum cues to the respondent so that he is given the maximum opportunity

to express *his* class identification perception, if any, rather than the investigator's perception of what the respondent's class identification might or should be. On this criterion, too, the open-ended approach is manifestly preferable.

Further, when the investigator offers the respondent a certain set or number of categories, he is open to the charge that another number or set of categories would be more appropriate. Thus, Cantril used five categories, Centers used four, and another investigator might use three or six. Which number is most acceptable? There is no clear cut logical or empirical answer to this query. The open-ended approach allows the investigator to bypass this problem completely in allowing the respondent to indicate his identification or lack of identification, and if there is identification, to use the interviewee's perceptions of his class affiliation or identification.

Finally, if class consciousness is a part of man's ego, a feeling on his part of belongingness to something,¹⁷ then it might be argued that as a minimum condition for its existence one should expect some kind of reasonable consistency in an individual's answers when replying to open-ended and closed type questions in regard to his class affiliation or identification. Such consistency was not apparent for a considerable body of the respondents in this inquiry.

There are a number of additional suggestive points to be noted in the tables. Space limitations preclude their full enumeration and discussion but the following should especially be noted: (1) the paucity of *upper* class designations regardless of the question asked; (2) the infrequency of *lower* class designations for all questions in all areas, except in Area D in Table 3; (3) the *relatively* heavy frequency of *middle*¹⁸ class designations for all questions, except in Area D; (4) the differential responses in the several residential areas dependent on the question asked. Several of these matters and their theoretical implications will be treated in a subsequent paper on stratification criteria and the perceptions of the "total class

¹⁵ There are probably more relevant techniques than those considered in this paper to ascertain the extent and kinds of class identification in a population. For the purposes of this paper, we arbitrarily limit our attention to the three techniques considered.

¹⁶ Richard Centers, *op. cit.*, p. 27.

¹⁷ *Ibid.*, p. 27.

¹⁸ See footnote a in Table 4.

structure," which paper will report in some detail the responses to the open-ended series of questions.

This study suggests the necessity of a more critical analysis of the conceptual definitions of class consciousness and class identifications and the research operations by which they are tapped. It also empha-

sizes that class identification findings are a resultant of the particular questioning technique employed. It is suggested that the open-ended question approach may be more appropriate in research dealing with class consciousness and class identification than predetermined class categories techniques.

TRENDS IN OCCUPATIONAL ORIGINS OF PHYSICIANS *

STUART ADAMS

Ohio State University

THIS paper is concerned with chronological and geographical variations in mobility into the medical profession.¹ Medicine is an object of special interest in this regard because it is an occupation which should, in effect, function as a "barometer" of stratification trends among the professions. The reason lies in the medical profession's unusual potentialities for structural and functional rigidity. It can be entered only through a formidable barrier of educational requirements which undoubtedly operates as a selective mechanism. The members of the profession are relatively easily controlled by the formal and informal organizations of the group. The profession is a powerful one, and it can and does exert tremendous pressure in defense of what the leaders of the group

consider its special interests. There are, finally, important pecuniary and prestige values to be conserved. If there are tendencies toward increasing rigidity in the professions of this society, the medical profession seems to be one of the most likely places for their appearance. Hence, this group should be an excellent testing ground for a number of hypotheses regarding stratification trends in this society.

PLAN OF THE STUDY

The present study is concerned only indirectly with the formulation and testing of mobility or stratification theory. Its primary task may be regarded as descriptive. It is an attempt to sketch the broader changes in the recruitment of members of the medical profession during the period 1900 to 1950. To trace these changes, samples of physicians born in consecutive chronological periods have been compared in direction and extent of change in mean occupational origin. In addition, these informants have been compared with respect to amount of direct occupational inheritance, and also with respect to degree of self-support in pre-medical and medical training.

In the first and most basic of these comparisons, the principal occupation of the informant's father was adopted as a primary reference point. This reference point served two functions: it defined the "occupational origin" of the informant, and it also provided a base from which the individual's vertical mobility could be measured.

To give the concepts of "origin" and

* A preliminary report on this study was presented at the 1952 meetings of the Ohio Academy of Science at Kent, Ohio.

¹ This report is part of a larger study of trends in occupational mobility in this society. Previous papers have dealt with aspects of mobility into the legal profession and into positions of business leadership. In future reports other professional occupations will be examined. Following the separate analyses, a comparative study of mobility in the several occupations will be presented. Earlier reports in this series include: "Regional Differences in Vertical Mobility in a High-Status Occupation," *American Sociological Review*, XV (1950), pp. 228-235, and "Business Size as a Factor in the Vertical Mobility of Business Leaders," *Proceedings of the Ohio Valley Sociological Society* (forthcoming). The writer wishes to acknowledge his gratitude to the Development Fund of the Ohio State University for assistance in some of the field work for these studies.

"mobility" greater operational utility, numerical scores were assigned to the father's occupations. These consisted of ratings taken from the North-Hatt scale of occupational prestige.² The occupational origin of an informant thus became the North-Hatt rating of his father's occupation, and the informant's mobility relative to his father's status position was the difference between the respective North-Hatt ratings.³ Here, as in other operations, the ratings served the obvious purpose of permitting more detailed and more precise statistical treatment than is usually possible with occupational mobility data.

THE SAMPLE

The subjects of this study were 333 physicians in four cities—Worcester and Springfield, Massachusetts, and Columbus and Toledo, Ohio. The sample was drawn from two geographical areas which differed in their social histories, particularly in their dates of settlement, with a specific purpose. It was anticipated that regional differences in origins would permit inferences regarding relatively remote vertical mobility rates. Such differences should reflect a greater span in the history of the profession than the two generations covered by the age difference among informants. This ought to provide some clues, indirectly, to the character of long-term trends in occupational origins within the profession.

The principal criterion applied in the selection of cities within a region was that of representativeness of the occupational profile of the male urban working population of the region. In addition, preference was given to localities falling within the 100,000 to 500,000 population range.

The original sample list included five

² Cecil C. North and Paul K. Hatt, "Jobs and Occupations: A Popular Evaluation," *Opinion News* (September 1, 1947), pp. 3-13. Ratings for occupations not listed in this scale were arrived at by interpolation, assisted by the use of other scales, including those of Edwards, Counts, and Deeg and Patterson.

³ Since the informants were all physicians, they were considered to be of equal occupational status. This assumption is required if the North-Hatt scale is to be used in the manner described. A number of methodological compromises are involved, but the procedure appears to be advantageous in an exploratory study of this kind.

hundred and fifty physicians. These individuals had been selected randomly from classified sections of telephone directories in the four cities. To compensate for differences in size of community, the Midwest samples comprised approximately three-fifths of the total.⁴

As an initial step in the collection of the data, questionnaires dealing with the basic life histories of the prospective informants were mailed to each of the 550 physicians. Approximately fifty per cent of the group returned the questionnaire. Of the remaining half, a one-fourth random samples was selected, and personal interviews were obtained with these physicians as soon as possible following the cessation of the mailed returns. The interviews covered the same ground as the mailed questionnaires, although additional information of a qualitative kind was gained incidentally during the interview.

Of the original sample of 550, a total of 261 usable mailed returns and 72 personal interviews were obtained. Ten of the questionnaires were discarded because of the absence of essential responses; of the attempts to secure personal interviews, three were refused.

The local characteristics of the sample, in mean occupational-origins scores, are shown in Table 1. For purposes of reference, it may be stated that the mean North-Hatt rating for ninety "typical" occupations is 69.8, and that the ratings range from 33 to 96. The rating for physicians is 93.

Two noteworthy features of the means in Table 1 are, first, the uniformity of the values for the mailed returns, and, second, the large differences between the mailed and interview means in the Northeast sample. Since these differences are statistically significant, it is evident that the mailed returns in this region were markedly biased in the direction of high-status informants.

To arrive at a working sample, one-fourth of the mailed returns were selected by a randomizing procedure and combined

⁴ This sample was not considered to be representative of the entire medical profession, although it was regarded as roughly representative of physicians in the larger urban centers of the Northeast and Midwest.

TABLE 1. MEAN OCCUPATIONAL ORIGINS OF PHYSICIANS IN FOUR U. S. CITIES

City	Type of Return	Number	North-Hatt Score	Standard Deviation
Worcester	Mail	53	79.9	8.5
	Interview	17	71.8	8.4
Springfield	Mail	53	78.9	8.0
	Interview	18	69.3	6.4
Columbus	Mail	88	79.0	7.1
	Interview	19	77.2	9.8
Toledo	Mail	67	77.8	13.2
	Interview	18	76.5	10.1
Total		333	77.8	9.6

with the interview returns. This was done in both the Northeast and Midwest samples. In the latter case it was necessitated not by the discrepancy between the two categories of returns but by the need to reduce the size of the sample proportionately. This avoided giving the Midwest group undue weight in computations in which the regions were combined into one sample.

TRENDS IN MEAN OCCUPATIONAL ORIGIN

The secular trend of occupational origins of the informants was determined by grouping origins scores into five year intervals, based on year of birth, and computing the interval means. These means are shown in Table 2.

TABLE 2. TREND OF OCCUPATIONAL-ORIGIN MEANS FOR PHYSICIANS IN FOUR U. S. CITIES

Date of Birth	Number	North-Hatt Rating	Standard Deviation
1875-1879	9	78.7	7.5
1880-1884	9	77.9	6.7
1885-1889	7	81.6	8.8
1890-1894	15	80.9	7.0
1895-1899	19	74.8	10.4
1900-1904	22	74.6	8.7
1905-1909	24	74.2	14.2
1910-1914	23	74.0	10.6
1915-1920	9	76.4	13.6
Total	137	76.1	10.7

Several matters in Table 2 are worthy of attention. First, there is a noticeable decline in occupational-origin means between the earlier and later periods in the series. The

trend is not linear but follows the general pattern of the logistic curve. If the sample is dichotomized at the beginning of the year 1895, the means for the earlier and later groups are 79.8 and 74.6, respectively. The corresponding standard deviations are 9.2 and 11.3, and the difference between the two means is significant at the .01 level of confidence.

A second matter of interest is the tendency for the standard deviations to increase with the passage of time. This indicates a broadening range of recruitment in the medical profession. Since there is little possibility of expanding the recruitment range into levels higher than that of physician, the broadening has consisted of the inclusion of more individuals from the lower-middle and lower classes.

A final point of interest is the apparent stabilization of the means in the period following the turn of the century.⁵ It would appear, generally, that for prospective physicians born in the years 1895 to 1915—i.e., individuals entering the profession between 1925 and 1945—there was relative stability of selection. Since half of this period consisted of an era which has often been considered conducive to rigidity in the professions, the medical profession seemingly has shown no pronounced internal tendencies toward restriction in the range of its recruitment in the present century.

⁵ The rise in origins in the last interval, though sizable, is not statistically significant. Because of this, and also because low-status individuals tend to be under-represented in the last time interval as a result of occasional interruptions of their training career, the rise is discounted in the present discussion.

DIRECT OCCUPATIONAL INHERITANCE

An alternative view of trends in occupational origins of physicians is presented in Table 3. In this table the percentage of

TABLE 3. NUMBER AND PERCENTAGE OF INFORMANTS WITH PHYSICIAN FATHERS, BY DECADE

Decade of Birth	Number of Informants	Informants with Physician Fathers	
		Number	Per Cent
1870-1879	9	2	22.2
1880-1889	16	4	25.0
1890-1899	34	4	11.8
1900-1909	46	4	8.8
1910-1920	32	3	9.4
Total	137	17	12.4

informants in the sample who had physicians for fathers is shown for each decade within the time span of the sample.

In Table 3, as in Table 2, there is evidence of a declining tendency for physicians to come from high-status families. There is likewise evidence that the tendency has apparently stabilized during recent decades. The slight percentage increase in the last decade has no significance in the present sample.

One point which also lacks statistical significance but which may be indicative of a real trend is the rise both in percentage of physician fathers and in occupational-origin means in the first two decades represented by these informants. If the trend is accurately portrayed, the data appear to mean that the profession has passed a peak in intensity of occupational inheritance and is currently in a period of decline from that peak.

TRENDS IN EDUCATIONAL SELF-SUPPORT

Since the expense of acquiring a medical education is one of the factors working for selective recruitment of candidates, it is relevant to inquire about the operation of this factor in the present sample. Responses to one of the items in the questionnaire made it possible to summarize the extent to which informants reported that they provided for their own college and medical-school training. Table 4 shows the percentage of self-support in each decade by total number and by region.

It is highly conjectural whether the estimates summarized in this table are reasonably accurate, since they involve the recall of fairly undramatic and somewhat unquantifiable materials from remote dates. Yet there are some consistencies in the figures, so they are presented as suggestive if not definitive.

The most conspicuous tendency in these data is that of a declining amount of self-financing in recent decades. This appears to indicate that the "democratization" of the medical profession in recent times has not been achieved by the simple process of increasing numbers of individuals working their way through school. Of the various possible explanations of this trend, two deserve mention here. One is the fact that a medical degree is becoming increasingly costly in terms of the candidate's time, so that self-financing by students is increasingly discouraged. Another is the fact that education appears increasingly accessible in this society, so that the professions may now be drawing candidates with superior educational resources or from lower social strata, or both. It is perhaps a significant fact that the region in which mean occu-

TABLE 4. TRENDS IN EDUCATIONAL SELF-SUPPORT BY PHYSICIANS

Decade of Birth	Number of Informants	Percentage of College and Medical Training Financed by Informant		
		Total Sample	Northeast	Midwest
1870-1879	9	53	37	63
1880-1889	16	58	41	66
1890-1899	34	44	45	43
1900-1909	46	33	23	42
1910-1920	32	28	12	37
	137	39	31	45

pational origins of physicians were lowest is the region in which less self-education was reported. These circumstances taken together seem to suggest that the trend toward decreasing self-support in school is not as yet an unfavorable development for the low-status aspirant to the medical profession.

REGIONAL DIFFERENCES IN VERTICAL MOBILITY

A number of regional differences are evident in several aspects of the foregoing data. Reference has been made to differences in educational self-support, in the manner of returning the questionnaire, and in mean occupational origins. These and other differences suggest that there may be appreciable inter-regional variations in mobility rates in the medical profession.

Possibly the most significant clue is the difference in occupational-origin means between the Northeast and Midwest samples. For the 61 individuals in the Northeast working sample, the mean occupational origin is 74.3, with a standard deviation of 8.8. For the 76 informants in the Midwest sample, the mean is 77.6, with a standard deviation of 10.9. The difference between these means is significant at the .05 level of confidence.

A number of inferences, important in the present context, may be drawn at this point. If the general occupational compositions of the two urban populations were comparable in recent decades, then it appears that vertical mobility into the medical profession has been more rapid in the Northeast. Also, if the state of the profession in the older regions tends to foreshadow developments in the newer regions, then the Midwest origins are likely to show further decline. Finally, if the declining origins in the profession are approaching an equilibrium, this equilibrium should be most apparent in the region of greatest decline. In the latter case, the Midwest should give evidence of a more rapid rate of decline in occupational origins at the present time.

The last inference is in fact supported somewhat by the coefficients of regression of the occupational-origin scores in the two regions. The coefficient for the Northeast sample is $-.06$, while that for the Mid-

west is $-.10$. The probabilities of these coefficients are .20 and .10, respectively, so their statistical significance is not securely established.⁶ In a summary interpretation, however, the regression coefficients appear to agree with the regional means in pointing to the Midwest as the most likely area of future decline.

One further suggestion that the Midwest may show more rapid decline in the future arises from the higher proportion of physician fathers among informants in that region. These proportions are 14.1 and 10.9 in the Midwest and Northeast, respectively. From tables 2 and 3 it is possible to infer that the ability of physicians to insure their own occupational inheritance is less pronounced than the ability of the profession to continue recruiting itself from among the offspring of high-status families.

DISCUSSION

The preceding sections indicate that individuals entering the medical profession during the period 1900 to 1950 have shown a significant tendency to become more representative of the gainfully employed male population of this society. This tendency is not overwhelming, since the mean occupational origin of the profession remains significantly higher than the mean prestige rating on the North-Hatt scale.⁷ And while the tendency may still be present, its most striking manifestation came in the earlier decades of this period. Recent decades have witnessed an apparent stabilization or near-stabilization of the recruiting level.

It may be surmised that the stabilizing influences include the depression of the 1930's and the dislocations of World War II. Both these events would have the effect of reducing the chances of low-status individuals to acquire the pre-medical and medical training necessary for entrance into the profession.⁸ There is, however, no clear

⁶ These probabilities become .10 and .05, respectively, when the coefficients are computed on the basis of 333 rather than 137 cases.

⁷ This mean prestige rating is, in turn, approximately ten points higher than the mean North-Hatt rating of U. S. workers as reported in the 1950 census.

⁸ The influence of war in this regard has received some recognition in the recent allegations

evidence that the depression had a perceptible stratifying effect upon this profession. There are, on the contrary, indications that medicine was less subject to stratifying tendencies during the depression than either law or big business.⁹ The leveling off in the recruiting for the former may be attributed in part to other factors, including the emergence of a new equilibrium between the formal requirements of the profession and the ability of the lower social strata to produce individuals who can meet those requirements at the present time.

If the depression was partly responsible for a modification of recruiting tendencies in medicine, it may be assumed that the effect was temporary. With the subsidence of this effect, further decline in the social or occupational origins of physicians may be anticipated. However, there are additional reasons for regarding the stabilization as a temporary phenomenon, and for anticipating a further decline in origins in the next decades. These are the G. I. bills of World War II and the Korean War, the growing pressure for an increased output of physicians in this society, and the growing availability of education. The latter development has a dual aspect. On the one hand, increasing accessibility results from conditions such as the rising standard of living of American families, the growing urbanization of the population, and an increasing emphasis on a relatively high level of education for every individual. On the other hand, there is a persistent drive toward the enlargement of professional training facilities.¹⁰

The higher occupational origins of informants in the Midwest appear to indicate that vertical mobility into the profession has

by public officials that "poor boys go to Korea and rich boys go to college."

⁹ A report currently in process.

¹⁰ Benjamin Fine, "Medical Colleges in Vast Expansion," *New York Times*, March 2, 1952.

been most effective in the Northeast. This phenomenon was shown to have been characteristic of attorneys¹¹ as well as physicians, and may be assumed to have resulted from (1) the greater availability of training in the Northeast, and (2) a higher motivation toward the professions in the eastern regions of this society. The latter tendency may be explained in terms of the ethnic compositions of the regions, differences in opportunity in the professions and other occupations, and possible regional differences in the interest value of the various occupations.

The democratization of the medical profession is not a wholly favorable development in the view of members of the profession. No systematic data were collected on attitudes toward present recruitment trends, but where sentiments were expressed by the interviewed physicians they were generally negative. It was commonly remarked that the contemporary trend fostered increasing commercialization of the profession. The responsibility was placed jointly upon the cost of entrance into medicine and the tendency toward recruiting more heavily from lower status levels, the latter defined mainly as "lower-middle" class. It was commented particularly that individuals who had acquired an education with considerable personal sacrifice tended to emphasize monetary values when they became established in the field. The relevance of this observation appears to be diminished somewhat by the decline in self-support in education, hence its interpretation is difficult. It is uncertain how much these observations are part of the defense of the group against "unsuitable" social types, and how much part of an ethical conflict within the profession.

¹¹ Regional origin means for attorneys were found to be 71.9 and 76.3 in the Northeast and Midwest, respectively.

TESTING MESSAGE DIFFUSION IN CONTROLLED EXPERIMENTS: CHARTING THE DISTANCE AND TIME FACTORS IN THE INTERACTANCE HYPOTHESIS *

STUART CARTER DODD

University of Washington

TESTING DIFFUSION—A CASE OF TRANSITIVE HUMAN INTERACTING

EXPERIMENTATION in Project Revere centers around, although it is not limited to, the testing of a highly general hypothesis of human interacting to be described below. It analyzes all human interacting or group phenomena into nine classes of observable factors which are expected to describe it well enough for predicting it. This experimentation is part of a contract with the Air Force to study the diffusion of messages in enemy, neutral, or friendly populations, resulting from leaflets dropped from planes. This diffusion is a verbal, one-way case of massive human interacting. It is communicating from person-to-person in large populations.

Our criterion of *interacting* within a group is the percentage of persons in a population who have learned from other people a given message that was started from leaflets. Our criterion of *reacting* of a plurel is the percentage of persons who learned a message

directly from mass media such as leaflets picked from the ground. The sum of these two percentages gives the percentage of knowers of the message, whether learned through physical or social channels. The percentage of knowers in a population is our chief index of diffusion, serving as an overall criterion for testing the nine classes of "factors" and eight classes of "conditions" into which we analyze the total situation.

This criterion-behavior of knowing the message (i.e., "diffusion") is often supplemented in our research by a second criterion-behavior of "acting-as-told" in the message (i.e., "compliance"). In these pretests we used simple all-or-none-messages of a sentence or so in length and simple all-or-none acts-as-told such as telling another person, passing on leaflets to others, mailing in a leaflet, and the like.

The interacting is analyzed as persons stimulating and responding to one another. More formal analysis yields the nine classes of essential *factors* and eight classes of residual conditions as: (1) actors, (2) acting, and (3) reactors, (4) reacting, (5) to external stimuli, and (6) internal stimuli (chiefly value attitude), under (7) temporal, (8) spatial, and (9) residual known and correlated conditions (of eight major kinds).

For convenience we subclassify the ninth factor, namely, the residual conditions, into the following eight dimensionally specifiable subclasses which often overlap: (a) stimulus conditions, (b) ecological conditions, (c) attitudinal conditions, (d) demographic conditions, (e) sociometric conditions, (f) organizational (mass media) conditions, (g) intercultural conditions, and (h) emergency (such as war) conditions.

The nine factors above are called the chief factor dimensions of human interacting. The experiments described below aim

* This study is one of a dozen reports on parts of "Project Revere," at the Washington Public Opinion Laboratory, financed by an unclassified contract with the Human Resources Research Institute of the U. S. Air Force. It represents one-third of the paper which was the program at the Sociological Research Association meeting in Atlantic City, September 1952.

For the author's previous publications on the interactance hypothesis, see *Systematic Social Science*, Seattle: University of Washington Bookstore, 1947, 789 pp.; "The Interactance Hypothesis—A Gravity Model Fitting Physical Masses and Human Groups," *American Sociological Review*, 15 (April, 1950); "A Measured Wave of Interracial Tension," *Social Forces*, 29 (March, 1951); "All-or-None Elements and Mathematical Models for Sociologists," *American Sociological Review*, 17 (April, 1952); "Testing Message Diffusion from Person to Person," *Public Opinion Quarterly*, 16 (Spring 1952); also S. C. Dodd and Henry Winthrop, "A Dimensional Theory of Diffusion," *Sociometry* (Spring 1953).

to chart the form and amount of dependence of the interacting, in these studies, upon two of these factors.¹ These two factors are: (1) the distance intervening between interactors—the spatial dimension, L ; and (2) the time involved in activity rates and growth curves—the time dimension, T .²

TESTING A DISTANCE FACTOR IN THE DIFFUSION PRODUCT

Among the aspects of diffusion studied was the dependence of the diffusion or inter-

¹ Further studies on these two factors, as well as on the other factors and conditions of interacting, may be obtained on request from the Washington Public Opinion Laboratory's list of mimeographed experimental designs, reports, and other documents on Project Revere.

² The relation of these essential factors of any human interacting and of their correlated conditions to each other can be specified in general in the dimensional formula below. This definitional formula defines the index of interactance, I_{pp} , as a mathematical product of powers of basic factors. The interactance is computed by raising each factor to some power as specified by its exponent and then multiplying it in some way with the other factors. In the simplified form here the further scripts, at the three other corners of each base letter representing a factor, have been omitted. These scripts specify the operationally defined indices, their units, origin points, limits, etc., which convert a dimensional formula (denoted by square brackets) to a statistical formula. Our "interactance hypothesis" expects a high correlation (e.g., a closeness of fit r , significant at the 5 per cent confidence level and exceeding $r=.71$) between the interactance index and an index of observed interacting. The interactance index in unit exponent form is defined as:

$$[I_{pp}] = \dots \text{the interactance or expected number of interacts (retellings of a message here) is defined as a product of}$$

$$k \dots \text{a constant adjusting the units used,}$$

$$\cdot P_A \dots \text{the number of actors (tellers here),}$$

$$\cdot A_A \dots \text{their acts (tellings here),}$$

$$\cdot P_B \dots \text{the number of reactors (hearers here),}$$

$$\cdot A_B \dots \text{their reacts (hearings here),}$$

$$\cdot T \dots \text{the duration studied (2 hours up to 2 months)}$$

$$\cdot L^{-1} \dots \text{the inverse distance intervening (between interactors)}$$

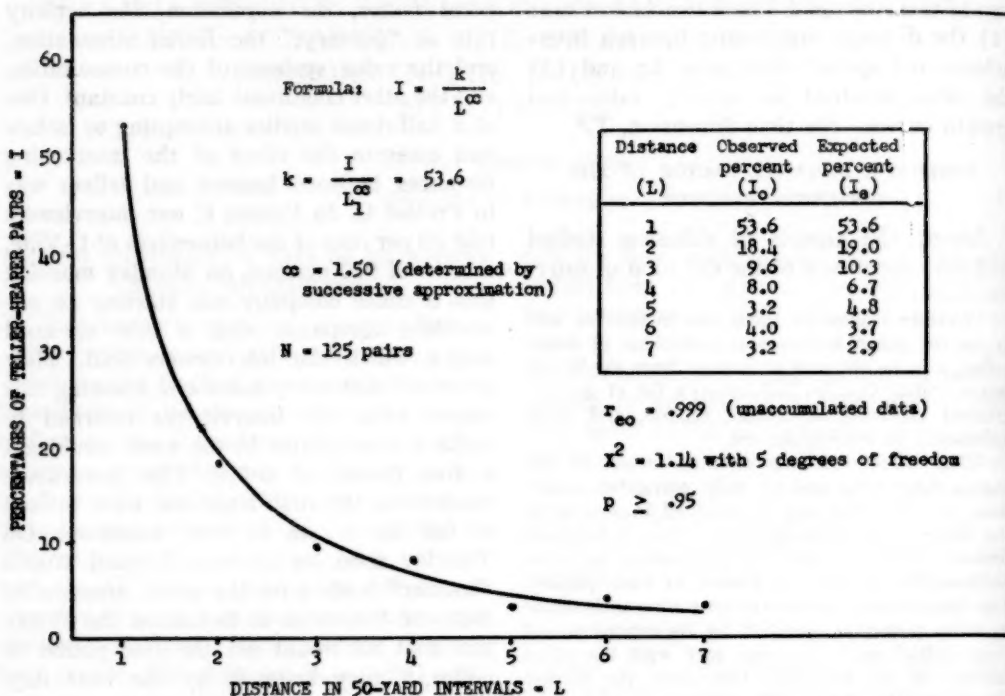
$$\cdot C_e] \dots \text{and their stimulus, attitudinal and other conditions not reported in this paper.}$$

The test of the distance factor reported below is a case isolating and confirming the kL^{-1} (for P^0 , A^0 , T^0 , C^0) in the formula above. The test of the logistic growth curve reported below is a case isolating and confirming the P_A , P_B , T factors as in Equation 1 (for A^0 , L^0 , C^0) in the interactance hypothesis above.

acting upon the distance intervening between hearers and tellers while keeping the temporal factor, the population, the activity rate or "potency," the leaflet stimulation, and the value systems of the communities, and the other conditions fairly constant. One of a half-dozen studies attempting to isolate and measure the effect of the intervening distances between hearers and tellers was in Pretest C. In Pretest C our interviewers told 20 per cent of the housewives of C-Ville, a town of 950 persons, on Monday morning that a coffee company was starting an advertising campaign with a new six-word slogan which the interviewers told. They promised that every housewife knowing this slogan when the interviewers returned to make a census later in the week would get a free pound of coffee. The housewives reached in the first interview were invited to tell the slogan to their neighbors. On Tuesday noon an airplane dropped 30,000 "booster" leaflets on the town, announcing that one housewife in five knew the slogan and that all would get the free pound of coffee if they knew it by the next day. This motivated non-knowers to seek out knowers whose knowing probably thus increased their prestige a bit. On Wednesday a census of households checked up on knowers, the time and place of telling, the tellers' and hearers' chains, and other sociometric data. From these data 184 pairs of matched hearers and tellers were mapped and studied. The distribution of their inter-home distances was made, first by hearers' reports as shown in Figure 1, and again by tellers' reports.

Previous exploratory efforts had indicated that the interacting waned with the distance. This means that a harmonic curve is the expected model or hypothesis. In cumulated form this is the logarithmic curve. In this pretest it was found that the skewed distribution fitted a harmonic curve excellently. Figure 1 shows the excellence of the fit of the data to the harmonic curve. The reliability of this is checked by observing these data twice—once by hearers' reports and again by tellers' reports. In both cases the correlation coefficients between data and model exceed our standard for the acceptability of a hypothesis. This standard requires a closeness of fit correlation index, significant at the 5 per cent level and exceeding

FIGURE 1. Distribution of Distances between Tellers' and Hearers' Place of Residence Fitted to an Harmonic Curve. Hearers' Reports in "C-Ville"



.9 in unaccumulated data. The r here was .999 between the observed data and the hypothesized curve.

At present, we interpret this to mean that the diffusing was a function of the least effort principle. If effort is defined as ergs of physical energy spent, then effort will vary directly as the distance walked. The intervisiting here was almost wholly by walking as the town was less than 500 yards in diameter. We assume people tend to conserve effort in getting what they want. The more the effort required, the less frequently will people make that effort if all else is equal. This least effort assumption is more exactly that the frequency of an act, when all else is constant, will vary inversely with the energy that act requires. Therefore, the diffusion will vary inversely with the distance which measures the effort required. Of course, the effort is not altogether at the moment of going to tell a friend about the free coffee. Effort was also involved in building up friendships at varying distances in the past. It seems likely that more friends would be cultivated nearby than far off. Thus communicating about the coffee

through friends may also be analyzed in part as involving the least effort principle.

TESTING A TIME FACTOR—THE GROWTH OF DIFFUSION

The time factor in interacting furnishes a good example of what we consider an ideal technique in sociological experiments which test models or seek laws. (A model is but an exact statement of a hypothesis in which all terms, conditions, assumptions, and their relations are explicitly stated.) The best "sociological model matching" in a well developed field of research: (1) starts with a description of the social conditions in the given field of inquiry; (2) then expresses these in mathematical symbols as the assumed mathematical conditions; (3) then derives the model mathematically from these assumptions exclusively; (4) then tests the closeness and reliability of the fit of this model to the social data; (5) and finally tries to test each social condition separately to check further whether or not these, and only these, assumptions hold in the given social situation.

Applying this ideal procedure to the time

factor in the field of interactance, let us list the simplest social conditions and their matching mathematical conditions below and see how some form of a logistic curve comes to be a resulting model.

To study human diffusion (the verbal, one-way interacting of people) we start with the following situation of maximal simplicity and regularity. We assume just three variables, namely: a large population, P ; an activity, A ; a time, T ; and just two relations between them,—random activity in the population, and steady activity in time.

The word "just" implies that all other factors and conditions are either constant or uncorrelated with the activity and therefore may be neglected in studying the growth of the activity. (This means, in the dimensional formula for interactance, that the distance (L) and other conditions (C) have zero exponents and so are unit constants.)

This closed system of just three dimensions develops growth laws of high generality. Thus if fruit flies are substituted for the population, Pearl's logistic laws of population growth result. If molecules are the population, the logistic growth curve of the self-exciting electric dynamo results. The larger and more homogeneous the population, the more smoothly probabilities will work out—although even a few hundred cases can yield close fits to logistic curves.

The activity is an all-or-none one, or may be dichotomized, for simplicity of study at first. It can be any act of a person to another person who is thereby "converted" or becomes a possessor of the attribute. The act we study is the telling-and-hearing of a message which divides the population at any moment into knowers and non-knowers. The model is general, however, to the diffusing of any novel behavior or attribute.

The time is the period of growth from the starting of a message until diffusing is finished—a matter of a few hours in our experiments to date in towns and schools. It may be measured in cardinal or clock units or in ordinal units of number of removes from the starters. Ordinal units have the advantage of eliminating daily and other cyclical influences which complicate a growth curve. They have an administrative disadvantage of requiring the

tracing of every link of who tells whom in each remove.

The conditions of random and steady activity may be altered with other models. Here they mean a normal distribution of the acts in the population and a rectangular distribution of the acts in time. This means a constant "potency" or activity defined by the hearers-per-teller ratio. This potency rate, or *acts per actor per period* rate, is the basic parameter and assumption needed in any growth model. It measures the potency of a given message to a given population in a given culture and situation. But once this rate of randomlike activity is specified, the logistic growth curve can be predicted to result irrespective of any other variables.

The randomness condition means, we believe, that everyone has equal social opportunity to interact with others. He need not actually interact with everyone. He may interact only with his own set, but if these sets of people are randomly interconnected with common members, the result would seem to be the same—though we have not tested this rigorously yet. The operational test of random interacting of any two proportions of a population is, by the law of joint probability, that the proportion of their interacts be the product of the two proportions. A chi square then tests the departure of the observed activity from the randomly expected activity.

This randomness condition needs much study and further specifying. At present we conceive of it as a binomial distribution resulting from many small uncorrelated influences. Thus, whether A tells B the message depends on many all-or-none influences such as whether A goes out today or not, whether B goes out or not, whether they meet or not, whether A knows the message or not, whether B knows the message or not, whether A thinks of it while talking to B or not, whether he thinks B would be interested in it or not, whether B keeps the conversation on other interests or not, and the like. A dozen such somewhat equal influences will tend to yield a normal distribution of the activity in a population. The statistical result will be equivalent to random interacting however purposive it may seem to the interactors.

The above description of the simplest

social situation for interacting is in terms (people, acts, and time) which are highly general to any culture or period of history. Such dimensional terms can develop social generalizations (or laws if empirically confirmed) which transcend the immediate situation, population, or culture.

Now expressing this baseline situation in mathematical terms, let:

p = the proportion of knowers of the message, p_0 being the "starters"

q = the proportion of non-knowers (so that $p+q=1$)

pq = the probability of knowers meeting non-knowers in a unit period of interacting

k = a function of the relative potency, or average number of persons told by a teller in a unit period divided by the total population. It is thus the probability (assumed constant in our research at first) of telling upon meeting.

Then the differential equation stating the time rate of growth is:

$$\frac{dp}{dt} = kpq \quad \text{Equation 1}$$

This equation says in prose terms that the increment in knowers in an increment of time varies with the potency (k) of the message time its number of active tellers (p) times its number of available hearers (q). Alternatively stated, the growth rate is dependent on the probability of knowers meeting non-knowers, pq , times the probability of telling-if-met, k , yielding the product probability of communicating.

Integrating this to cumulate these increments in successive periods, the result is the linear logistic curve defined by:

$$p_t = \frac{1}{1 + q_0/p_0 e^{-kt}} \quad \text{or} \quad \frac{p_t}{q_t} = \frac{p_0}{q_0} e^{kt} \quad \text{Equation 2}$$

This is an S-shaped growth curve, shown in Figure 2, resembling the normal ogive. It can be thought of as the "most probable" growth curve under the simple conditions sketched above. It tells the proportion of knowers, p_t , at any time, t , growing from the starting proportion p_0 . Its slope or general steepness is fixed by the potency, k , since $k/4$ is the slope or tangent at the mid-point and mid-date (as shown by substituting $p_t = .5$ in Equation 1).

This Equation 2 is deduced from Equation 1 and states the mathematical logistic law. We then experimentally tested in the field to discover how closely the actual social conditions conformed to the assumed conditions of random and steady interacting in a large population. We made half a dozen pretests until we learned in Pretest F how to design a social situation whose conditions could be specified in advance and observed in the field and matched closely by the mathematical conditions of the appropriate model.⁵

On Pretest F we hypothesized that the growth of diffusing would follow a linear logistic curve. This means that we predicted that a linear logistic form of curve would fit the data well without predicting its parameters—which could not be expected in a wholly new sort of situation never before observed. The hypothesis specifies the form but not the amounts of growth.

To match social and mathematical conditions we chose a school population of 184 seventh-graders as being highly homogeneous and spatially close together. We motivated them with a prize contest to interact by trading messages. Each pupil was given 33 cards, one containing a complete message, while the other 32 cards lacked two or three words each. These missing phrases had to be obtained from a pupil who knew it either from his own complete card or from earlier tradings.

⁵ The chief experimental difficulties were: (1) to keep the potency constant—most messages wane in interest, thus requiring models in which k is a variable; (2) to keep the growth trend free of daily rhythm and other cyclical overlaying influences, either by using ordinal units of successive removes or by shortening the overall period down to a few hours, or by lengthening it to a week with a day or other cycle as the unit, or by finding an activity, a group, or a situation which is little influenced by such cycles; (3) to find messages of interest to all the population and not limited to a segment—to keep the upper asymptote of the curve the same as the census population; (4) to develop social diffusion unmixed with physical diffusion direct from the mass stimulation of the leaflets—this meant testing the logistic model cleanly, unmixed with an exponential model.

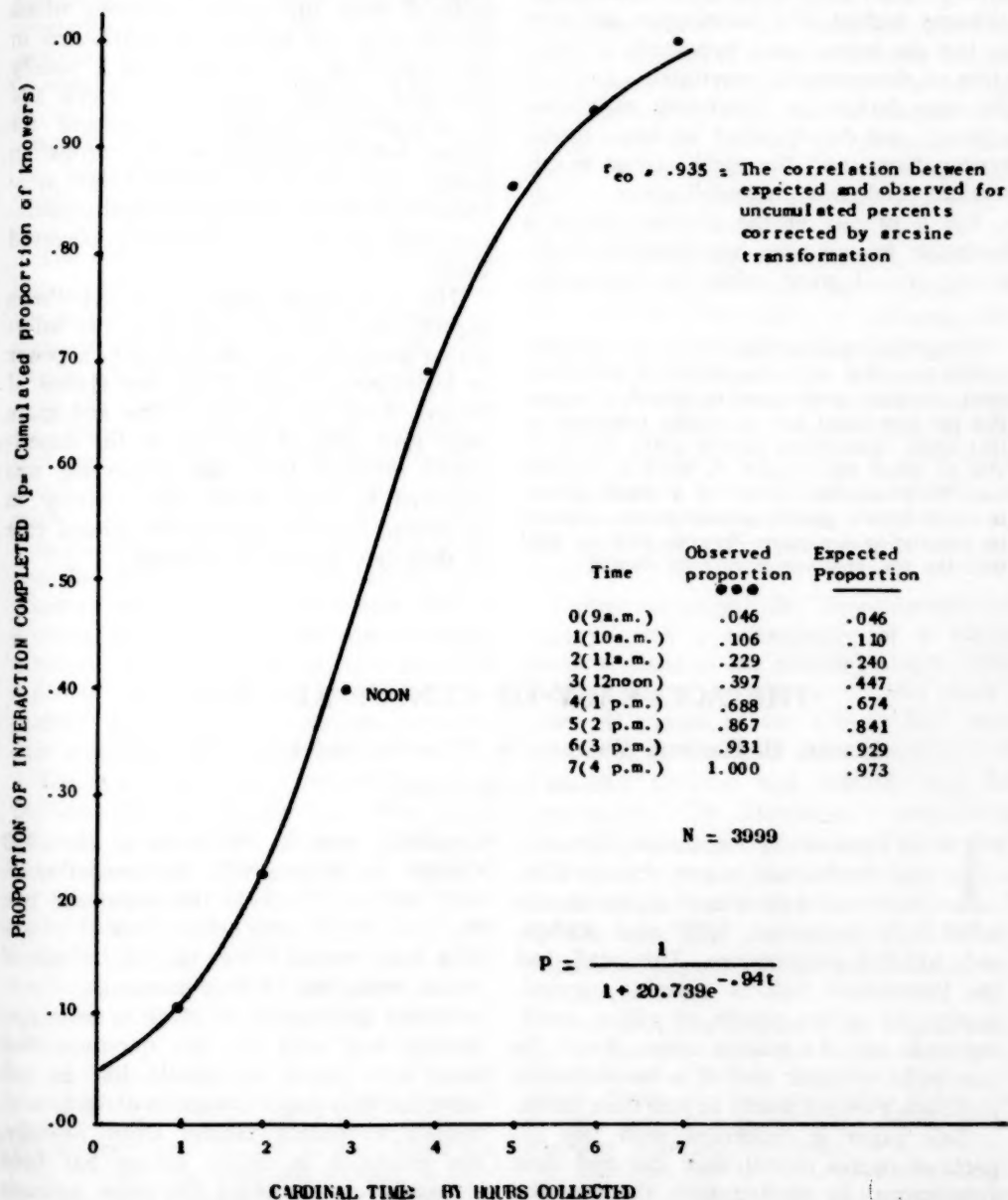
Each of these difficulties was overcome experimentally. More important, however, models for these complicating conditions have been developed as a result of discovering experimentally what the complicating conditions happen to be.

They traded enthusiastically for two days, completing most of it on the first day. They dropped their completed cards in collection boxes, so that with hourly collections the growth of diffusion could be tabulated and checked by the times they asserted on the cards. The cards recorded the tellers for each of the 5,666 tellings (from 33 cards and 184 pupils) so that

sociometric chains were traced and growth of interacting could be tabulated by removes also.

The test of the logistic hypothesis here is shown in Figure 2. The fit is close and good. The correlation index between the observed data and the logistic expectation in uncumulated form was .95 (and .99 in cumulative form). Both are significant at

FIGURE 2. Testing the Logistic Hypothesis for the Growth of Diffusing by the Interacting of a Group of Persons to Each Other



the five per cent level. We therefore conclude that the linear logistic hypothesis was confirmed *under the conditions specified here*.⁶

SUMMARY

The controlled experiments on interacting in Project Revere at the Washington Public Opinion Laboratory pursue two objectives equally. For the Air Force, we seek principles to maximize the desired response of any target population to stimulation by airborne leaflets. For sociologists, we seek to test the interactance hypothesis or principle of demographic gravitation. Each of the nine factors or dimensions of "interactance" are being tested in turn, before testing them, and the eight classes of attendant conditions, in combinations.

For a pretest of the *distance factor* a harmonic waning was hypothesized—interacting should wane with the intervening

distance between tellers and hearers if other things are equal. A coffee advertising slogan was started among 20 per cent of the housewives in a town with free coffee promised for those knowing it later. The message spread to 82 per cent of the housewives in 2 days. The closeness of fit of data to this harmonic curve of intervening distance was a correlation of .999,

For a pretest of the *time factor*, S-shaped logistic curves for interacting were predicted as a rational hypothesis mathematically *deduced* from the social conditions which specify how the activity is distributed in the population and in time. In a totally new and different situation in which 184 seventh-graders traded 33 messages for prizes, reliable closeness of fit, correlation indices over .95 in uncumulated data were observed between the hypothesized expectation and the later experimentally observed facts.

The dimensional interactance hypothesis expects that the diffusion here (or interacting generally) will depend upon a power or logarithm of each of its nine classes of factors. When the factors of time and space were each isolated in turn in the experiments reported here, the interacting was observed to vary closely and reliably in agreement with the appropriate special case of that very general hypothesis.

⁶ This does not preclude better fits by other models here. Nor does confirmation in one experiment constitute confirmation in general, of course. But the experiment here is readily repeatable so that wider confirmation can be tested. It should also be noted that Pretest F, which is reported here for illustration, is one of a dozen pretests in which logistic growth principles were explored in preparation for more rigorous tests in 1953 with the pre-conditions more fully charted.

THE ACCURACY OF CENSUS RESULTS

MORRIS H. HANSEN, WILLIAM N. HURWITZ, and LEON PRITZKER

Bureau of the Census

THE Censuses of Population, Housing, and Agriculture, taken during 1950, required the services of approximately 1,000 field supervisors, 9,000 crew leaders, and 132,000 enumerators. This staff had the tremendous task of covering approximately 151 million people, 46 million dwelling units and 5.4 million farms. From the standpoint of sheer size, it is inconceivable that such a census would be free from errors.

This paper is concerned with two aspects of census errors: their size and their significance. In dealing with the question of size, we shall describe an attempt at

measuring some of the errors of the 1950 Census. In dealing with the second question, we shall indicate the important role that sociologists and other users of census data must assume before the significance of census errors can be fully assessed.

Before attempting to deal in any systematic way with the two questions that have been posed, we should like to call attention to a major change in attitude with respect to census taking. Until recently, the emphasis in census taking has been primarily on producing the most accurate census possible, without any particular at-

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tention being given to the question of the required accuracy and to what extent it is worth an additional cost to increase the accuracy. Attempts to evaluate the cost of achieving different levels of accuracy in comparison with the possible costs arising from wrong decisions resulting, in turn, from errors in statistics were stimulated by the introduction of modern sampling methods. In the use of these methods emphasis has been placed on achieving results of needed accuracy at minimum cost and on attempting to consider the overall accuracy required in relation to the costs and the purposes to be served. The objective of "optimum design" in sampling is coming to be the objective in census taking. Instead of striving for perfection, we view the task as that of balancing the costs of producing statistics against the losses from errors in the statistics.

The first topic to be discussed is that of the size of census error, more specifically, the methods and results of an attempt to measure the extent of error in the 1950 Census. It is important at the outset to point out that, if major national aggregates for population, housing, and agriculture characteristics were all that were desired, there would be no need for a complete census. Recent developments in sampling indicate that important national aggregates can be estimated from a sample with accuracy equal to or greater than that of a census at a fraction of the cost of taking a census. A census, however, is required in order to provide data for very small geographic areas or for cross-classifications that involve relatively few people.

The fact that a small well-designed and well-administered sample can yield more accurate measurements than is feasible in the much larger scale operations required for a census suggests the use of the former as a vehicle for checking on the latter. In fact, the Bureau of the Census and other statistical agencies have used this second approach to study the accuracy of censuses.¹

¹ See: Gabriel Chevy, "Control of a General Census by Means of an Area Sampling Method," *Journal of the American Statistical Association*, Vol. 44 (1949), pp. 373-379. A. Ross Eckler and Leon Pritzker, "Measuring the Accuracy of Enumerative Surveys," *Proceedings of the International Statistical Institute*, New Delhi, India, 1951.

Such sample evaluations started on a systematic basis in the United States with the 1945 Census of Agriculture and continued with the 1947 Census of Manufactures and the 1948 Census of Business.

In the 1950 Censuses of Population, Housing, and Agriculture, a major project was undertaken on a sample basis to evaluate both the completeness of coverage and the accuracy of response to selected inquiries for the units (persons, dwelling units, farms) included in each Census. This project was called the Post-enumeration Survey (or PES) and had two major purposes. The first purpose was to provide a measure of accuracy of the results of the Census in order to aid the consumer of Census data in avoiding the drawing of inferences that would be unwarranted by the accuracy of the data. The second purpose was to ascertain sources of error and the size of error associated with each source. Such information, supplemented by evidence on the cost of eliminating or reducing the causes of error, provides information needed to improve census design. Thus, costs of achieving various levels of accuracy can be balanced against the requirements of the users of the data. Obviously, if there were no problem of increased cost in achieving a perfect result, we would try for perfection, but as a practical matter this is not the case.

Calling the project the "Post-enumeration Survey" was a consequence of a major decision made in the planning stages. This decision was to conduct the "quality check" after all phases of the Census had been completed in the field. Some discussion of how this decision was reached may be appropriate:² The alternative to conducting the "quality check" after the Census that was considered was that of conducting the check at the same time as the Census.³ In line with this alternative, the Current Population Survey carried out monthly by

² For an account of some of the decision making for the Post-enumeration Survey, see Marks, Mauldin, and Nisselson, "A Case History in Survey Design, the Post-enumeration Survey of the 1950 Census," to appear in the *Journal of the American Statistical Association*.

³ There was also the third (and meaningful) alternative of conducting the check prior to the Census.

the Bureau of the Census was considered as the vehicle for the check. In fact, this Survey was used to throw light on the accuracy of some of the more dynamic inquiries included in the Census, particularly labor force participation.

The Current Population Survey was also considered as the basis for checking the accuracy of other characteristics such as the coverage of the Census, the age distribution, the income distribution, the pattern of educational attainment, the tenure status of dwelling units, the uses of farm acreage, and so on. One of the main advantages of using the Current Population Survey for conducting a quality check at the same time as the Census is that the effect of memory bias on the results of the quality check itself is eliminated. However, there were some major disadvantages in the use of the Current Population Survey as a concurrent quality check. Any quality check procedure which could be construed as a "whitewash of the Census" would lose public acceptance. It was thought by many that comparisons of Census information with sample information for households included in the monthly sample, or even from other households selected in the same sample areas as were used in the Current Population Survey, might "condition" the Census results. With this sample the areas to be checked would be known in advance to many of those participating in taking the census, and such knowledge might have the effect of increasing the quality of the census in the areas that were to be checked. Also, the areas in the Current Population Survey might, on the whole, have better trained supervisors and enumerators than other areas. It was thought that these factors might lead to understatement of the differences between the Census and the quality check.

It was decided, therefore, that it would be desirable to conduct the quality check for characteristics other than labor force participation after the Census was taken. It was decided also not to disclose the locations of the quality check sample areas until the Census was completed. Moreover, the decision was made to assign quality check supervisors and interviewers to areas other than those in which they worked during the Census.

There were other reasons for not conducting the quality check at the same time as the Census (or even immediately following the completion of the Census in a given area). These reasons had to do with the desirability of making available to the quality check interviewers the records of the Census enumeration for the sample areas to be checked. We could find no feasible way of doing this such that the quality check could be carried out at the same time as or immediately following the Census in a given area. In our view, ability to study many characteristics does not deteriorate with time, as, for example, the determination of age. The decision in favor of a *post*-enumeration survey was predicated in part on the hypothesis that the effect of memory bias is not severe for many items. For example, whether one interviews to ascertain last year's income during April and May or during August and September may not lead to significantly different results because of the passage of time. We need further evidence on this hypothesis, since it contradicts many widely held opinions.

Thus, the decision was made to conduct a *post*-enumeration survey. In order to describe more fully the methods of the PES we shall limit the discussion to its use in the Population Census.

A major aim of the PES was the measurement of *net coverage error*. This measure is obtained by making estimates of two other measures. The first is the estimate of people not included in the Census who should have been included (gross under-enumeration in the Census), and the second is the estimate of names included in the Census that should not have been (gross over-enumeration in the Census). The net under (or over) coverage error is the difference between these two.

Another major objective was the evaluation of the accuracy of responses recorded on the Census schedules for persons included in the Census, in an effort to study age, income, occupation, and other characteristics. Here, the interest was in studying the extent of the *net bias in reporting* as well as of gross error in reporting. It should be clear that we can have errors of over-enumeration and under-enumeration and still have no net coverage error. In

the same way, a particular characteristic such as age may have no great net bias in reporting, that is, the over-reporting of age may balance the under-reporting. The PES was designed to measure both such gross and net errors.

To achieve these objectives the sample for the Post-enumeration Survey was divided into two major parts. The first part was a sample of areas, and the second was a sample of dwelling units from the Census. The sample of areas was to be re-canvassed intensively in order to locate any dwelling units in each area that were not included in the Census but should have been. The sample of dwelling units drawn from the Census returns was used in making three estimates: (1) the number of persons not included in the Census, who lived in dwelling units that were properly enumerated; (2) the number of persons enumerated in these dwelling units, who should not have been included in the Census; and (3) the accuracy of response to selected questions included in the Census.

It should be pointed out that the coverage evaluation was made in relation to Census enumeration districts. That is, we regarded a person as left out of the Census if he was not enumerated in the Census enumeration district in which he should have been enumerated, even though he might have been included elsewhere. We regarded a person as included in error if he was included in a Census enumeration district where he should not have been, even though he was not included anywhere else. Thus, the gross errors are stated in relation to the areas defined for taking the Census because it was with respect to these areas that the rules were laid down as to who should and who should not be counted by each enumerator. The effect of determining the gross error in coverage for enumeration districts is to over-state this error for areas larger than enumeration districts. The expected net coverage errors are not overstated, however.

For the evaluation of the accuracy of response to Census inquiries, the PES was limited to selected items. Among the items studied in the check on the Population Census were age, income, occupation, educational attainment, migration, and birth-

place. For most of these items a more intensive approach to questioning was introduced in the Post-enumeration Survey than was feasible in the Census itself. For example, the Census asked in order to determine "wage and salary" income for a person, "Last year (1949), how much money did he earn working as an employee for wages and salary?" The PES, on the other hand, used a battery of questions designed to uncover all the jobs a person held in 1949 and the gross income from each job.

Other steps taken in the Post-enumeration Survey to improve accuracy, not only of response on subjects covered in the Census, but on the coverage check itself, included the following:

- (1) The personnel for carrying out the PES was selected from among the best enumerators and crew leaders who had worked on the Census. There were three criteria: (a) a high score on selection tests, (b) excellent performance in the Census insofar as performance could be evaluated, and (c) recommendations by the field supervisors.
- (2) PES interviewers were given special training, far more intensive than was possible in the Census itself.
- (3) Instead of being paid on a piece rate basis, as in the Census, the interviewers were paid a daily rate to get away from any possible incentive to sacrifice quality.
- (4) It was specified in the PES that data be obtained from the best respondent for each question, as distinguished from the situation in the Census, where the housewife frequently answers for other members of the family. It is often presumed that one of the problems in the Population Census arises in the collection of statistics on occupation and industry, for example, because the data are frequently collected from the housewife instead of from each worker individually. In the PES steps were taken to ask the question of the best respondent, and a great amount of effort was expended in pursuit of this best respondent. In the case of youngsters and certain incompetents this best respondent would be the mother or some other specified member of the family.

We have outlined a number of very important changes in procedures, method and emphasis, in the Post-enumeration

Survey as compared to the Census, that should lead to a presumption that the PES will yield more accurate results. However, it should be recognized that this is only a presumption. The PES and the Census are both subject to error; this must be kept in mind in any interpretation of the results.

In fact, the PES was far from a perfect instrument for measuring the size of Census errors. This is seen, for example, by the results of a small-scale check made on the PES itself. This "check on check" revealed missed dwelling units that were not detected by the PES interviewers. Also, there were errors in drawing the sample and in processing the results. Finally, the PES results on the numbers of missed persons classified by age do not accord with some evidence from other sources.

Although there were important errors made in the PES, it appears quite reasonable to assume that the quality of work was such as to provide a standard of comparison for the Census that was in many important respects more reliable than the Census itself.

We have introduced a series of "record checks," moreover, to provide evidence on the accuracy of response in the PES as compared with response in the Census. For example, a check on income reports in the Census is being made against Bureau of Internal Revenue income tax returns. Veteran status reports to the Census and to the PES are being compared with the records of the Veterans Administration. Ages reported in the Census and the PES are being compared with birth records and with earlier Census reports. Also, a check is being made of industry reports and wage and salary income reports in the Census by comparison with the records of the Bureau of Old Age and Survivors Insurance. These record checks are carried through on a matched basis, person by person. For the names that are successfully matched, a comparison is made of the information in the agency's record with that from the Census and the PES. Presumably, more accurate information is contained in such records.

Those who have attempted to carry through such matching studies will be immediately aware of the very great difficulty of matching the information from one

source such as the Census against that from another source such as Social Security records. The existence of dubious matches as well as of unmatched cases makes it difficult to draw any direct inferences concerning the accuracy of the Census. We have assumed, however, that these independent records could be effectively used to evaluate the PES results as compared with the original Census returns by examining only those cases in which reasonably positive matches have been established. Thus, it is assumed that comparison, for only the matched cases, of the PES reports and the Census reports with the independent data provide a basis for testing the hypothesis as to the relative accuracy of the PES and Census reports.

Thus far in our discussion of the study of accuracy of the Population Census we have outlined some of the methods used in the PES, some of its limitations and, finally, a proposed technique for "validating" the PES (the record check). Now, let us consider some of the results. Only the measures of coverage error are available at this time. The total omissions of persons from the Census in enumeration districts where they should have been enumerated was 2.3 per cent with a standard error of 0.2 per cent. The number enumerated who should not have been was 0.9 per cent with a standard error of 0.1 per cent. This leaves a net omission rate of 1.4 per cent plus or minus 0.2 per cent, or about 2 million plus or minus 340,000 people. Differences between age groups were not as striking as were expected. As between urban and rural, the underenumeration seems to be somewhat greater in the very large cities and the more rural areas. The curve appears to be U-shaped, with lower omissions in the smaller size cities. The net relative undercoverage of non-whites was about two and a half times that of whites.

Knowledge of the sources of the discrepancies in coverage provides a basis for considering further action. For example, techniques for dealing with the problem of underenumeration can arise from learning that about two-thirds of the persons erroneously omitted from the Census were in households where the entire household was missed and about a third were missed

within enumerated households. Within enumerated households the highest rates of underenumeration were among people who were not members of the immediate family, such as lodgers, servants, fathers- or mothers-in-law, and similar groups. Apparently the probability of being missed increases with the remoteness of the relationship to the family.

It is important to note that the PES measures of underenumeration do not include persons who live in hotels and in institutional places. The problem of underenumeration in such living quarters is being studied, too, and the results will be reported subsequently. For this and other reasons, it is reasonable to regard the estimates above as indicating a lower limit on the net underenumeration in the Census. Some of the people whom the original Census takers missed may also have been missed by the PES interviewers. It appears, on the other hand, that the PES may be expected to have identified overenumeration more adequately, since the names of those included in the Census were on hand. The PES needed only to verify their existence and the appropriateness of their inclusion by the Census enumerators.

Persons erroneously included are of two types: names that should not have been listed anywhere, and persons who have been enumerated in the wrong enumeration district. Names that should not have been enumerated anywhere include any "padding" or fabrication of the records that may have taken place. We were particularly interested in this latter figure. We were pleasantly surprised that the estimates of padding by the Census enumerators amounted to less than a tenth of one per cent of the enumerated United States population. This is a far smaller figure than some of the people working with the Census had feared, after having seen the individual cases uncovered and the prosecution of enumerators and others in the isolated instances that came to attention.

Nearly all of the names that were listed in error were those of persons who should have been enumerated in other enumeration districts. These account for the bulk of overenumeration, or about 0.8 per cent, out of the total 0.9 per cent enumerated in error. They include people who had claims

to two or more residences for one reason or another. The problem of describing the appropriate place to enumerate people who travel and work, or have no usual place of residence or maintain two or more places of residence is a most difficult one. The problem is made more difficult because the Census enumerator is paid on a piece rate, which creates an incentive for him to get as many people as he can.

Until now, we have been considering the problem of how we have gone about measuring the errors in Census statistics, and some results of the measurements made, and not of what accuracy requirements should be imposed on Census results. What should our goal be, and how can we arrive at it? What is the significance of census errors? This is a problem deserving of very careful attention and consideration by groups such as the American Sociological Society as well as the Bureau of the Census. Until recently, it is safe to say that the philosophy of the staff at the Bureau of the Census, and in general of the advisors to the Bureau and of most users of Census data, was that we should strive for perfection and try to get the most accurate measure possible of anything that we were trying to measure. The subject of the relationship of accuracy to the utility of the results has not received much serious attention in the literature until very recently, and much of what has been written is in terms that are difficult to apply to practical problems. Very often people have considered the question of accuracy and have not considered the question of utility, and conversely the question of utility has been discussed in a conceptual setting rather than in terms of the accuracy of measurement of a particular statistic used for particular purposes. We believe that a solution of the problem calls for, among other things, recognition of the principle that considerations of accuracy and of utility are inseparable. We have been forced to this recognition by having to view the Bureau of the Census as a factory engaged in the production of statistical tables. In the management of that factory, just as in the management of any other factory, it is necessary to make, in some sense, the input of the factory comparable with its output. Management is required to examine the

costs of production and relate them to the value of what is produced. The value of our product depends on the uses that are made of it and, depending on the use and the risks and losses associated in making a mistake in the use, the allowable tolerances of error may vary. Instead of the principle of highest possible accuracy, then, this view substitutes the principle of determining the level of accuracy that is optimum by balancing the losses due to errors against the costs of greater accuracy.

Considerations of this sort have already led to considerable modification in some of the methods in Census work. The Bureau has, by design, reduced the accuracy of compilation, where errors are subject to close control and were on a near zero basis. Higher error tolerances have been introduced in processing work, controlled at specified levels through quality control or sample verification and other techniques. The considerable economies which have resulted have made it possible to take additional steps to try to improve the quality of the field work, where error levels are considerably higher.

In order to achieve desired levels of accuracy three kinds of information are required. All three kinds require the extension of the idea of using a measurable design, developed in modern sampling theory, to the entire survey or Census process. The three kinds of information can be characterized as follows:

- (1) Knowledge about the cost and accuracy of alternative methods of conducting the same survey.
- (2) Knowledge about the accuracy actually achieved in a given survey.
- (3) Knowledge about the accuracy required for a given survey. This in turn requires knowing how the statistics are to be used and what the risks and losses associated with inaccuracy are.

On the first two points we are beginning to amass a body of techniques which help to tell us something about the accuracy associated with our methods and the accuracy of given results. On the last point we must, at least at this stage of our development, rely largely on our collective intuitions and on the implicit evaluation made by the Congress through setting ap-

propriations. With respect to the accuracy of alternative methods, we use the pretest and pilot study, and have initiated a series of research projects to test alternative procedures and to estimate the cost associated with them. For example, a major test was carried through in the recent Census comparing the use of self-enumeration versus direct interviewing to collect Census data in the fields of population, agriculture, and housing. We are comparing methods of mark sensing with other methods of recording information. We are studying different methods of compilation. We are working on the development and application of electronic computing equipment. Similarly, we are studying the effect of the contribution of the interviewer to response error. Also, we are trying to develop methods of quality control for field operations that are as satisfactory as those for operations in the processing of the returns.

Results from such activities should make it possible to come closer to the ideal of an optimum design for a sample survey or a census.

A necessary condition for progress, however, is to obtain better answers to the question, "What accuracy should we try to buy?" Perhaps that is stating it the wrong way. Perhaps we should ask, "What are the losses associated with differing levels of accuracy?" Suppose, for example, we could succeed in changing the completeness of the Census from a net underenumeration of 1.4 per cent to a net underenumeration of, say, 0.4 per cent, and that we could specify what it costs at each fraction of a percentage point along the way to achieve that level of accuracy. To what extent is the additional accuracy worth the price and to what extent is it not? The same types of questions arise in measuring employment, marital status, fertility, and other characteristics. Frequently by the use of appropriate methods and the expenditure of additional resources we can use measurement methods such that the results will converge closer and closer to the true value of the characteristics being measured, although they will not necessarily approach it with an error close to zero for any plausible budget.

What, then, are the levels of accuracy

that are justified by the cost required to achieve them? The advisors to the Census and the users of Census and other statistical data need to give more and more attention to such questions if we are to serve adequately our function of collecting and making available useful and adequate statistics. The cost of achieving the level of accuracy which we now attain is appreciable. In this last Census, for example, we introduced a hotel check in which all persons staying in hotels were enumerated on "T night" (night for enumeration of transients), no matter where the hotels were located nor where the guests had their usual residence. Then the records of this enumeration were taken back and checked against the guests' usual place of residence to see if they were enumerated there. This innovation of the 1950 Census cost about an extra million dollars. Also, it added about 700,000 people (0.4 per cent) to the Census. Was it worth this cost? Other steps of a similar nature could be taken. They might be costly and also time consuming in the Census operation. Any major gains that might be accomplished at little or no cost should obviously be taken, but this whole area of the relation between cost of achieving different levels of accuracy and the effect of different levels of accuracy on the utility of results is of the greatest importance.

Among the more specific types of decisions that we must face in this connection is the answering of questions such as: Should we have carried through and should we, in the next Census, carry through a

hotel check similar to the one just described? Should we pay an additional price for a Census in order to shorten the period of the Census?

More generally the following questions must be answered:

- (1) Is it more important to have more statistics (that is, statistics on a large number of subjects or more detailed statistics on the same subject) than to have more accurate statistics?
- (2) Is it more important to have more statistics or to spend funds in obtaining measurements of the errors of the statistics already obtained?
- (3) Is it more important to use funds in increasing the accuracy of statistics? If so, how can we know to what extent we are effective?

These are difficult questions. As a practical matter, such questions have to be answered. Even to approach consideration of these questions there must be a presumption that we can measure not only sampling errors but also response errors, errors in compilation, and perhaps what might be called errors due to the types of measures selected (errors arising from the choice of inappropriate definitions, for example).

Some progress has been made in answering the three questions that we have posed simply by the efforts to measure errors in statistics. The study of the significance of Census errors can only be advanced in the long run, however, when sociologists, economists and others who use Census results face up to their responsibilities.

MARITAL HAPPINESS OF PARENTS AND THEIR CHILDREN'S ATTITUDES TO THEM

PAUL WALLIN
Stanford University

AND

HOWARD M. VOLLMER
United States Army

THIS is a report of findings from a group of questionnaire studies which add considerable confirmation to "the often verbalized but infrequently substantiated relationship between the subject's conception of his parents' marriage and his feeling toward his parents."¹ Specifically, the present article is concerned with the association between ratings by men and women of the marital happiness of their parents and (a) the extent of their attachment to each parent and (b) their parental preference.

NATURE OF THE DATA

The data of this report are drawn from a series of studies which collected information on parental happiness and the attitudes of men and women to their parents. Utilizing essentially the same question, the studies all required their subjects to rate the marital happiness of their parents. They also asked the subjects to rate their attachment for father and mother, although in this matter—as will be shown later—the form of the question varied somewhat in the different researches.

The separate ratings of attachment for father and mother made it possible to classify the subjects, in each of the studies, according to parental preference by comparing the amount of attachment indicated for the male and female parent. Subjects were classified on this basis as (a) neutral, stated degree of attachment the same for both parents, (b) preferring mother, greater attachment being indicated for her than for father or, (c) preferring father, the greater attachment being reported for him. Parental

preference as thus established, and extent of attachment for father and mother taken separately, were examined in each of the studies for their association with the ratings of the marital happiness of the parents.

SOURCES OF THE DATA

The sources of the data were (1) Burgess and Wallin's study of engaged couples, (2) their study of married couples, (3) Vollmer's study of parental preferences of Negro and white college students and (4) a study by Wallin of the delinquencies of college students.

The Burgess-Wallin research on 1000 engaged and 600 married couples² can for our purposes be regarded in a sense as separate studies, despite the fact that the latter group was part of the former. There was an interval of about five years between the initial and the later phase of the investigation. Moreover, the questions on marital happiness of parents and on the subjects' attachment to them were not identical in the two stages of the research. The engagement questionnaire called for a rating of the happiness of the parents' marriage on a five category scale ranging from "very happy" to "very unhappy." Attachment to parents was registered in response to two items relating to mother and two parallel items for father. The items as presented in the questionnaire were "Check your attitude toward your parents on the following scales":

- (a) Your attitude toward your mother when you were a child (check): very strong attachment, considerable attachment, mild attachment, mild hostility, considerable hostility, very strong hostility.

¹ Robert F. Winch, "Interrelations Between Certain Social Background and Parent-Son Factors in a Study of Courtship Among College Men," *American Sociological Review*, 11 (1946), p. 334.

² For a detailed description of this research see Ernest W. Burgess and Paul Wallin, *Engagement and Marriage*, J. B. Lippincott Co., 1953.

- (b) Your present attitude toward your mother (check): categories same as above.
- (c) and (d) Same questions and response categories as above, for father.

In the marriage study subjects were asked to "rate the marital happiness" of each parent³ on a nine point scale starting with "extraordinarily happy" and ending with "extremely unhappy." Attitudes to parents were reported for mother and father separately in response to the following items:

- (a) Amount of conflict before marriage between you and your father (check): none, very little, moderate, a good deal, almost continuous.
- (b) Amount of attachment before marriage between you and your father (check): none, very little, moderate, a good deal, very close.
- (c) and (d) Same as above, for mother.

The men and women in the engagement and marriage study can be briefly described⁴ as volunteer subjects who were white, native born, and residents of metropolitan Chicago. The great majority were of college level and approximately half of them were Protestant with most of the others reporting Jewish or Catholic affiliation. At the time of the engagement research four fifths of the women and two thirds of the men were under 25. At the time of the marriage study 75 per cent of the couples had been married between three and five years. In filling out the engagement schedule about a third of the persons exercised their option of remaining anonymous. In answering the marriage schedule all subjects were aware of the fact that their identity was known.

The data of Vollmer's study⁵ were secured by means of an anonymous questionnaire administered to classes in three colleges⁶ in

the San Francisco Bay area and four Negro educational institutions.⁷ The classes were for the most part in sociology or psychology.

Vollmer used the returns from 223 white males, 399 white females, 175 Negro males and 160 Negro females. With a few exceptions they were undergraduates. The median age of both Negroes and whites was twenty. The sample studied was limited to unmarried persons between 16 and 27 years of age, whose parents were both alive. The subjects were predominantly Protestant. In a minority of cases one or both parents were foreign born. The parents of about 55 per cent of the white subjects had one or more years of college education; the parents of about one third of the Negroes were of this educational level. A considerably greater proportion of the Negro mothers than of the white mothers were engaged in part time or full time work. The Negro and white students differed further in that the latter were more likely to have had one or no siblings and the former two or more.

The parental happiness item in Vollmer's study was "In your opinion, up to the time you were 12, were your parents on the average happy or unhappy in their marriage? (Check) very happy, happy, average, unhappy, very unhappy." Information on parental attachment was secured with two questions relating to mother and two identical questions relating to father. The first question was "When you were a child, before the age of 12, did you love your mother (check): very much, considerably, somewhat, a little, not at all." The second question, using the same response categories, was "At present, do you love your mother?"

The last study which provided data for the present research was concerned with the delinquencies of college students.⁸ The anonymous questionnaire used in this project elicited ratings of parental happiness⁹ in reply to the question "In general, how happy was your parents marriage during the first 16 years of your life?" The response

³ There was a very marked correspondence between the ratings of marital happiness of the two parents. Consequently only the happiness rating given fathers has been used in our analysis.

⁴ For a detailed statement about the characteristics of the sample, see Burgess and Wallin, *op. cit.*, Chapter 2.

⁵ See Howard M. Vollmer, *Variations in Parental Preference of Negro and White College Students*, unpublished M.A. thesis, 1951, Stanford University Library.

⁶ San Francisco State College, Stanford University, and University of California.

⁷ Atlanta University School of Social Work, Clark College, Fisk University and Morehouse College.

⁸ Unpublished study by the senior author.

⁹ In this as well as in the other studies parents who were separated or divorced were placed in the lowest category of the happiness rating scale.

categories were those used in the Burgess-Wallin engagement schedule and by Vollmer. The attitude of subjects to their parents was obtained with the item, "Amount of attachment between you and your mother until age 16 (check): none, very little, moderate, a good deal." This was followed by the parallel item for father.

The delinquency questionnaire was filled out by 89 men and 73 women registered in two consecutive classes in a Criminology course taught by the senior author. The students in these classes were predominantly

ness of parents and a parental preference on the part of their offspring.

The coefficients, of course, do not reveal the pattern of association between the variables under consideration. The pattern in all instances was very similar to that shown by the data of Table 2. Three consistent trends are apparent in the data. First, the happier the rating of the marriage of parents, the greater the probability that persons will report the same degree of attachment for both parents. Thus 59 per cent of the men who rated their parents' marriage

TABLE 1. COEFFICIENTS [†] OF CONTINGENCY BETWEEN PARENTS' MARITAL HAPPINESS AND PARENTAL PREFERENCE OF OFFSPRING IN SPECIFIED AGE PERIODS

Study	Age Period	Men		Women	
		C	N*	C	N*
Burgess-Wallin (Engaged couples)	Childhood	.25	968	.24	954
	Engagement	.33	857	.36	843
Burgess-Wallin (Married couples)	Before marriage**	.27	560	.23	557
Vollmer (Negro group)	Before 12	.39	172	.33	158
(White group)	Before 12	.17	219	.39	397
(Negro group)	College	.35	172	.40	159
(White group)	College	.28	222	.44	396
Wallin (Delinquency study)	Until 16	.27	89	.34	73

[†] The coefficients of the various studies are not comparable with respect to magnitude since the tables from which the coefficients were computed differed in the number of their cells. (Adjacent categories were sometimes combined to satisfy the chi-square condition that there be a minimum expected frequency of 5 in each cell. In a few instances this condition was not met.) The coefficients for men and women within the individual studies are, however, comparable.

* Number of cases in the distribution from which C was calculated.

** The item utilized here for establishing parental preference was "Amount of attachment before marriage."

juniors and seniors majoring in the various social sciences. They were largely unmarried, in the age range 18 to 21, Protestant, and of native born parents most of whom had one or more years of college education.

FINDINGS

Table 1 summarizes in the form of 16 coefficients of contingency the findings from the various studies on the association between marital happiness of parents as rated by their children and the latter's parental preference as previously defined. Thirteen of the 16 distributions yielded chi-squares significant at the .001 level. The chi-squares of two of the other distributions were significant at the .01 and .05 levels. These results establish rather conclusively the fact of an association between the marital happi-

as "very happy" fall in the "neutral" category in contrast to 23.7 per cent who characterized their parents relationship as "unhappy" or "very unhappy." The second trend revealed in the distribution is that the less happy the parents are rated as being, the greater the likelihood that more attachment will be indicated for mother than for father. Whereas only one third of the men who rate their parents' marriage as "very happy" are partial to their mothers this is true of two thirds of those who judge their parents' union to be "unhappy." Third, it can be noted that at *all* levels of marital happiness a greater proportion of men and women indicate more attachment for their mothers than for their fathers.

The pattern described above was found in the 16 distributions of Table 1. In all

TABLE 2. CHILDHOOD PARENTAL PREFERENCES OF 968 MEN AND 954 WOMEN AS RELATED TO MARITAL HAPPINESS OF THEIR PARENTS *

Happiness of Parents	Parental Preference							
	Men				Women			
	Neutral	Mother	Father	N	Neutral	Mother	Father	N
Very happy	59.0	32.4	8.3	339	56.8	28.0	15.3	347
Happy	44.9	47.8	7.3	301	53.4	35.9	10.7	262
Average	31.2	58.0	10.8	231	33.9	49.6	16.5	230
Unhappy**	23.7	66.0	10.3	97	27.8	55.7	16.5	115

* From Burgess-Wallin study of engaged couples.

** Ratings of "unhappy" and "very unhappy" combined. The category also includes divorced or separated parents.

but two cases the magnitude of association was almost identical for male and female subjects. The exceptions were the white students in Vollmer's study.

We turn now to the findings on the relation between marital happiness of parents and the attitudes of their offspring to each parent considered independently. Table 3 indicates an association in all groups studied. The chi-squares for all distributions relating to men's and women's attachment to father were significant at the .001 level. Eight of the 14 chi-squares for the distributions relating to attachment to mother reached this level of significance. Four of the remaining six were significant at the .05 level or higher.

The findings, then, are decisive in regard to the association between parental happiness and the reports by men and women of the extent of their attachment to the male parent. The findings for the female parent are consistent but somewhat less de-

cisively reliable. Nonetheless, they can be accepted with considerable confidence.

The data of Table 4 show the *pattern* of association between the ratings by engaged men and women of the happiness of their parents' marriage and reports of their present attitudes to their fathers. The pattern is clear and consistent: "very strong attachment" to father is far more likely to be reported when parents' marriage is rated "very happy" than when it is rated "unhappy." "Mild attachment" on the other hand, tends to be characteristic of persons who rate their parents' marriage as "unhappy." The trend is very similar for men and women.

Table 5 reveals the same pattern of association in regard to attitude to mother. The magnitude of the correlation is smaller, however, than that obtained for attitude to father. The generality of this difference can be noted in the coefficients of Table 3. For

TABLE 3. COEFFICIENTS * OF CONTINGENCY IN INDICATED STUDIES BETWEEN PARENTS' MARITAL HAPPINESS AND EXTENT OF ATTACHMENT OF OFFSPRING TO EACH PARENT IN SPECIFIED AGE PERIODS

Study **	Age Period	Father Attachment				Mother Attachment			
		Men		Women		Men		Women	
		C	N	C	N	C	N	C	N
B-W (Engaged couples)	Childhood	.41	976	.39	960	.27	976	.22	974
	Engagement	.50	891	.54	873	.35	945	.35	940
B-W (Married couples)	Before marriage	.40	565	.45	564	.29	577	.37	574
V (Negro group)	Before 12	.39	172	.36	156	.20	175	.14	160
(White group)	Before 12	.32	220	.39	396	.27	223	.22	399
(Negro group)	College	.40	174	.49	157	.22	175	.21	160
(White group)	College	.40	223	.49	396	.33	223	.25	399

* See footnote † of Table 1 regarding the comparability of the coefficients of the various studies. The statements made there also apply to the coefficients of the present table.

** Studies are identified by their authors' surname initials. (The data of Wallin's delinquency study were not analyzed for attitude to individual parents.)

TABLE 4. PRESENT ATTITUDE TO FATHER REPORTED BY ENGAGED MEN AND WOMEN AS RELATED TO PARENTS' MARITAL HAPPINESS *

Happiness of Parents	Attitude to Father							
	Men's Attachment				Women's Attachment			
	Very Strong	Considerable	Mild **	N	Very Strong	Considerable	Mild **	N
Very happy	58.6	34.3	7.1	309	62.8	30.1	7.0	312
Happy	31.7	48.2	20.1	278	29.6	52.1	18.3	240
Average	16.7	35.6	47.6	216	20.9	34.9	44.2	215
Unhappy †	12.5	20.5	67.1	88	9.4	19.8	70.7	106

* From Burgess-Wallin study of engaged couples.

** The responses "mild attachment," "mild hostility" and "very strong hostility" were combined for this table.

† The ratings "unhappy" and "very unhappy" are here combined. The category also includes divorced or separated parents.

TABLE 5. PRESENT ATTITUDE TO MOTHER REPORTED BY ENGAGED MEN AND WOMEN AS RELATED TO THEIR PARENTS' MARITAL HAPPINESS *

Happiness of Parents	Attitude to Mother							
	Men's Attachment				Women's Attachment			
	Very Strong	Considerable	Mild **	N	Very Strong	Considerable	Mild **	N
Very happy	66.7	29.1	4.2	330	73.2	22.9	3.8	340
Happy	43.1	48.3	8.6	290	48.8	45.2	6.0	252
Average	33.5	43.5	23.0	230	43.9	39.1	16.9	230
Unhappy †	33.7	40.0	26.3	95	39.8	33.1	27.2	118

* From Burgess-Wallin study of engaged couples.

** The responses "mild attachment," "mild hostility" and "very strong hostility" are combined in this category.

† The ratings "unhappy" and "very unhappy" are here combined. The category also includes divorced or separated parents.

TABLE 6. RELATIVE AMOUNT OF CONFLICT WITH FATHER AND MOTHER BEFORE MARRIAGE AS RELATED TO PARENTS' MARITAL HAPPINESS *

Parents' Happiness	Amount of Conflict							
	Men				Women			
	Same with Both Parents	More with Father	More with Mother	N	Same with Both Parents	More with Father	More with Mother	N
Extraordinarily happy	81.0	12.1	6.9	58	71.4	10.0	18.6	70
Decidedly happy	64.8	18.9	16.4	159	56.3	17.2	26.6	128
Happy	52.9	20.0	27.1	140	54.8	21.2	24.0	146
Somewhat happy or average	38.1	36.7	25.2	139	35.0	30.8	34.2	117
Less than average **	31.3	35.8	32.8	67	28.9	32.0	39.2	97

* From Burgess-Wallin study of married couples. Coefficient of contingency for men .30, for women .27. Chi-squares for both distributions are significant at .001 level.

** Combination of ratings ranging from "somewhat unhappy" to "extremely unhappy." The category also includes divorced or separated parents.

MARITAL HAPPINESS OF PARENTS

429

TABLE 7. AMOUNT OF CONFLICT WITH FATHER BEFORE MARRIAGE REPORTED BY HUSBANDS AND WIVES AS RELATED TO PARENTS' MARITAL HAPPINESS *

Parents' Happiness	Amount of Conflict							
	Husbands				Wives			
	None	Very Little	Moderate or More	N	None	Very Little	Moderate or More	N
Extraordinarily happy	57.6	30.5	11.9	59	70.8	20.8	8.4	72
Decidedly happy	53.7	30.2	16.0	162	60.0	28.5	11.5	130
Happy	49.3	29.3	21.4	140	51.0	30.0	18.8	149
Somewhat happy or average	34.3	34.3	31.4	140	33.9	31.4	34.7	118
Less than average **	28.4	16.4	55.3	67	34.0	25.8	40.2	97

* From Burgess-Wallin study of married couples. Coefficient of contingency: for husbands .32, for wives .32. Chi-squares for both distributions are significant at .001 level.

** Combination of ratings ranging from "somewhat unhappy" to "extremely unhappy." The category also includes divorced or separated parents.

all groups and age periods the marital happiness ratings correlate more highly with attachment to father than with attachment to mother. The mean of the 7 coefficients for the distributions bearing on father attachment is 40.3 for the men and 44.4 for the women; in the case of mother attachment the means are 26.9 and 25.1 respectively.

Finally it is interesting to note the relationship between the marriage happiness ratings and the amount of conflict with parents reported by the men and women. Data on this were available only for the subjects of the Burgess-Wallin marriage study.

Table 6 shows the association between parental happiness and the difference in "amount of conflict before marriage" with father and mother, as reported by the sample of husbands and wives. In one respect the pattern of association indicated by the percentage distributions parallels that

found for the attachment relationship to parents. The parallel is that the happier the parents' marriage is rated, the greater the probability that both men and women will report the same degree of conflict with father and mother. The parallel, however, does not hold beyond this point. The decline in the ratings of parental happiness does not have associated with it a lesser degree of conflict with mother than with father. As parental happiness declines, increasing proportions of men and women indicate "more conflict" with one or the other parent. But neither parent figures markedly or consistently in the trend. There is some indication that independently of parental happiness men tend to have more conflict with their fathers and women more with their mothers.

The data remaining for consideration are those bearing on the association between the marital happiness of the parents and the

TABLE 8. AMOUNT OF CONFLICT WITH MOTHER BEFORE MARRIAGE REPORTED BY HUSBANDS AND WIVES AS RELATED TO PARENTS' MARITAL HAPPINESS *

Parents' Happiness	Amount of Conflict							
	Husbands				Wives			
	None	Very Little	Moderate or More	N	None	Very Little	Moderate or More	N
Extraordinarily happy	60.0	30.0	10.0	60	56.2	31.5	12.3	73
Decidedly happy	51.8	34.5	13.7	168	51.5	31.5	16.9	130
Happy	41.8	33.3	24.9	141	46.6	29.1	24.3	148
Somewhat happy or average	38.8	36.0	25.1	139	26.9	37.0	36.1	119
Less than average **	28.6	24.3	47.1	70	30.8	26.0	43.3	104

* From Burgess-Wallin study of married couples. Coefficient of contingency: for husband .30, for wives .29. Chi-squares for both distributions are significant at .001 level.

** Combination of ratings ranging from "somewhat unhappy" to "extremely unhappy." The category also includes divorced or separated parents.

extent of the children's premarital conflict with each parent taken separately. Table 7 presents the evidence on men's and women's conflict with their fathers. An inverse relation between marital happiness of parents and extent of conflict with father is clearly operative for men and women subjects. In both groups the odds are about 5 to 1 that individuals who rate their parents' marriage lower than average (as compared to those who rate them extraordinarily happy) will report a moderate or greater degree of conflict with father. The distribution of percentages in Table 8 reveals much the same pattern in men's and women's premarital conflict with their mothers.

SUMMARY OF THE FINDINGS

The findings which have been reported here are comparable with some results obtained by Winch in a courtship study of 435 college men and 502 women.¹⁰ Winch's sample was more homogeneous than any of those used in our analysis. His subjects were from 16 middle western coeducational colleges and universities having chapters of national fraternities and sororities. They were between 19 and 25 years of age and their mothers and fathers were living together. Negro, Oriental, Jewish and foreign-born individuals were excluded.¹¹

Winch utilized the responses to a number of items to score his subjects on their degree of love for each parent.¹² His findings, however, are for the most part in agreement with ours. Among the men and women parental happiness was correlated positively with love for father and love for mother. And for both sexes parental happiness was

negatively correlated with difference in degree of love for mother and father.

There is one apparent discrepancy between the results of Winch's investigation and those of the present study. In the former, parental happiness correlated significantly higher with love of father than with love of mother among the men subjects only.¹³ In the latter this difference was consistent for men and women.

The analysis of data from a series of studies of Negro and white college students offers strong support for the validity of the following propositions for the universe of American families from which the subjects were drawn:

1. The happier men and women rate their parents' marriage, the more likely they are to report the same degree of attachment in the past and at present for both parents.
2. The less happy the parents' marriage is rated, the greater the probability that men and women will indicate greater attachment in the past and at present for their mothers than for their fathers.
3. There is, however, a positive correlation between parental happiness and extent of attachment to each parent.
4. At all levels of parental happiness men and women are more likely to report stronger attachment in the past and at present for their mothers than for their fathers.

The findings in regard to the association between parents' marital happiness and their offspring's conflict with them must be regarded as tentative since data on this question were available in only one of the studies examined for this report. The time period at issue was "before marriage." Briefly, the findings were that:

1. There is an inverse correlation between the parents' marital happiness and the amount of conflict men and women subjects report with each of the parents.

¹³ Winch states that in the case of the men his findings "suggest that when the parents' marriage is a happy one, there is a tendency for the son to prefer his father, and when it is unhappy for him to side with his mother" (p. 334 of the article cited in fn. 1). That this inference is not warranted is indicated by the finding of the present study that, despite the fact that at all levels of parental happiness men and women tend to report more attachment for mother than for father, the association of parental happiness with extent of love for father is greater than with love for mother.

¹⁰ See footnote 1 for a citation on the study of college men. For the study of women see "Courtship in College Women," *American Journal of Sociology*, LV (November, 1949), pp. 269-278. P. 272 of this article reports the results of Winch's study of the reliability and validity of the items "attitude to parents" and "parental happiness." The reliability and validity of marital happiness ratings are discussed at length in Chapter 3, *Predicting Success or Failure in Marriage*, E. W. Burgess and L. S. Cottrell, Prentice-Hall, Inc., 1939.

¹¹ See "The Relation Between Courtship Behavior and Attitudes Towards Parents Among College Men," *American Sociological Review*, 8 (1943), pp. 164-74.

¹² *Ibid.*, pp. 167-168.

2. With decline in parental happiness, increasing proportions of men and women report a difference in amount of conflict with father and mother, but neither parent figures consistently in the trend.

INTERPRETATION OF THE FINDINGS

The association between marital happiness of parents and the attitudes of their offspring to them in childhood and later periods can be plausibly interpreted. Research has demonstrated that the marital happiness of husbands and wives is correlated with their attitudes to children, whether they have them or not. The unhappily married are more likely than the happily wed to indicate a negative attitude to children.¹⁴ This, in part, may account for the association between parental happiness and the love of children for their parents. Whatever the reason for the correlation between marital happiness and attitudes to children, the latter is obviously an important variable in the interaction between parents and offspring. Genuine love and acceptance—or dislike and rejection—of the child by the parent undoubtedly tend to evoke similar feelings in the child.

A second explanation of the association between parental happiness and attitude of children to each of their parents rests on the assumption that the unhappiness generated by marital failure tends to impede the development or maintenance of a harmonious well-balanced child-parent relationship. The mother or father who is unhappily married may experience tensions and frustrations which influence behavior toward the child. The parent may be irritable, unduly severe, inconsistent or overprotective to a

degree which materially decreases the child's affection and increases the extent of parent-child conflict.

A simple interpretation suggests itself for the tendency of men and women to indicate relatively greater attachment for mother than father as the level of parental happiness declines. It requires the assumption that children are likely to impute greater responsibility to their fathers than to their mothers for the marital unhappiness of their parents. The mother spends much more time with the children and is in a better position to convey to them a conception of herself as the aggrieved party in the unhappy marital relationship. This devaluation of the male parent would dispose the offspring of unhappy unions to be somewhat more partial to the female parent. In happy marriages, on the other hand, the mutual affection and esteem of husband and wife may well facilitate development in the child of a conception of both parents as equally lovable.

The preceding argument also suggests a possible answer to the question of why parental happiness is correlated more highly with men's and women's attitudes to their fathers than with their attitudes to their mothers. If the mother's evaluation of the father is a determining factor in the child's attitude to the father, then, insofar as the mother's evaluation is a function of the happiness of the marriage it could be expected that love of father would vary more than love of mother with parental happiness. The fact of there being *some* association between the parents' marital happiness and the child's attitude to mother may be accounted for by one or both of the explanations given above for the relation between parental happiness and the attitudes of children to each of their parents.

¹⁴ See the chapter, "Children and Marital Success," in the volume cited in footnote 2.

OFFICIAL REPORTS AND PROCEEDINGS



REPORT OF THE COMMITTEE ON NOMINATIONS AND ELECTIONS

The Committee, appointed by President Samuel A. Stouffer, consists of the following members: J. Howell Atwood, Howard W. Beers, Jessie Bernard, Ernest W. Burgess, Guy B. Johnson, Forrest LaViolette, Alfred McClung Lee, Calvin F. Schmid, William H. Sewell, Conrad Taeuber, Dorothy Swaine Thomas, Preston Valien, Malcolm M. Willey, Robin M. Williams Jr., and Kimball Young. As stipulated in the Constitution the personnel of the Committee is broadly representative of the membership of the Society, taking into account both the fields of their specialization and their geographical distribution.

The nominees for officers, committee member and Council members, were selected after four ballots during the period beginning October 1, 1952 and ending January 26, 1953. The fourth ballot was, however, only for concluding the nominations for president-elect, since the other nominees had been selected on the third ballot.

The ballot mailed out from the Executive Office on February 19, 1953 presented the following nominees:

President-Elect

Maurice R. Davie
Donald Young

First Vice-President

Jessie Bernard
Lowry Nelson

Second Vice-President

Philip M. Hauser
Ira deA. Reid

Member of Committee on Publications

Donald J. Bogue
Otis Dudley Duncan

Members of the Council

Gordon W. Blackwell
Margaret Jarman Hagood

Rudolf Heberle
Everett C. Hughes
Harvey J. Locke
William H. Sewell
Fred L. Strodbeck
Preston Valien

Spaces on the ballot were provided for members to fill in the names of additional nominees. A total of 87 nominations were made by members for the different positions, but no person received a sufficient number of votes for a particular office to require a second election under the conditions stated in the Constitution.

The tellers appointed by the chairman of the Committee were Winston W. Ehrmann, T. Lynn Smith and O. Bruce Tomason, all of the University of Florida. With the aid of assistants they conducted the count of the ballots after the names and addresses on the envelopes had been checked against an official list of members of the Society. Only three ballots were found to be invalid, one because the person was not on the official membership list and two because no name had been signed on the outer envelope. The total number of valid ballots was just short of one thousand (994).

The persons elected are:

President-Elect

Donald R. Young

First Vice-President

Jessie Bernard

Second Vice-President

Ira deA. Reid

Member of Publication Committee

Otis Dudley Duncan

Members of the Council

Gordon W. Blackwell
Margaret Jarman Hagood
Everett C. Hughes
Harvey J. Locke

ERNEST W. BURGESS, *Chairman*

NEWS AND ANNOUNCEMENTS



United Nations Plans for the World Population Conference. The Economic and Social Council of the United Nations in a resolution dated 10 June 1952 approved "the holding of a world population conference of experts in 1954, under the auspices of the United Nations, in close collaboration with the International Union for the Scientific Study of Population and interested specialized agencies." The Council decided "that the conference shall be devoted solely to the exchange of ideas and experience on population matters among experts in the field concerned." The Secretary-General was requested "to invite, in their individual capacity, experts nominated by (1) governments, (2) non-governmental scientific organizations concerned and (3) the interested specialized agencies; and in addition to invite a small number of experts with a scientific interest in population questions."

The time of the conference has not yet been definitely fixed, but it is expected that it will be held in August 1954.

A Preparatory Committee has been established to assist the Secretary-General in formulating a program and in making the necessary arrangements for the conference. This Committee consists of representatives of the United Nations, interested specialized agencies and the International Union for the Scientific Study of Population, and a few internationally recognized population experts. At its first meeting, which was held in Geneva in November 1952, the Committee elected Mr. P. K. Whelpton of the United Nations as its Chairman, and Mr. P. Depoid of the International Union for the Scientific Study of Population as its Secretary. Upon the advice of the Committee a Sub-Committee on Arrangements has been established with Mr. F. Lorimer as Secretary.

The Preparatory Committee drew up a preliminary list of topics of general interest for discussion sessions. This list of topics is appended to the present statement together with a list of examples of additional topics of special interest which might be discussed by small technical groups at the conference.

Invitations to individual experts to take part in the conference will be issued by the Secretary-General of the United Nations upon the nomination of governments, non-governmental scientific organizations and interested specialized agencies. Invitations to present papers at particular sessions and to participate in panel discussions will be arranged by an organizer for each session who will be designated by the Secretary-General in consultation with the Preparatory Committee. Each person taking part in the conference will have the opportunity to submit a scientific communication up to 4000 words in length, on a matter relevant to population. The maximum length of papers to be presented upon invitation of the organizers for various sessions will be 8000 words.

Questions relating to the program and to facilities for the participation of individuals may be addressed to the Secretary of the Sub-Committee on Arrangements: Professor Frank Lorimer, American University, Washington 16, D. C., U. S. A.

UNESCO has accorded Consultant Status last month to the World Association for Public Opinion Research. This means that for advice on any international questions involving public opinion research, surveys, or pollsters the United Nations organizations will turn to WAPOR as representing the polling profession throughout the world. At the moment WAPOR has contracts with UNESCO for two international surveys—one on international polling itself, and one on a before-and-after study of change of attitude resulting from an educational campaign in three nations on the Declaration of Human Rights. The consulting may be focused through the secretary of WAPOR who is Professor Stuart C. Dodd, Director of the Washington Public Opinion Laboratory in the Department of Sociology of the University of Washington at Seattle.

Fulbright Awards. Persons interested in these awards should watch mailed announcements and other sources of information than the *Review*. The announcement in 1953 did not reach the editorial office until March 30—much too late for the information to reach readers for the April 15 deadline.

Lecturing and teaching appointments for 1953-54 are to be made for posts in Germany, Finland, Australia, Burma, Ceylon, India, New Zealand, Philippines, Thailand, and Union of South Africa. The 1954-55 programs for Austria, Belgium and Luxembourg, Denmark, Egypt, Finland, France, Germany, Greece, Iraq, Italy, Japan, the Netherlands, Norway, Pakistan, Sweden, and the United Kingdom and Colonial Dependencies will be announced in June 1953. Application forms and additional information are obtainable from Conference Board of Associated Research Councils, Committee on International Exchange of Persons, 2101 Constitution Avenue, Washington 25, D. C.

John Hay Whitney Foundation. The designation of six Whitney Visiting Professorships in the Humanities and Social Sciences for 1953-54 was announced recently by the John Hay Whitney Foundation. The recipients were selected from among the most distinguished recently retired professors in America. The appointments call for a year's residence by the individual professors at small, independent liberal arts colleges. It is expected that in addition to actual classroom teaching the Visiting Professor's time will be used in consultation with the faculty and students with particular attention being devoted to students of the freshman and sophomore years. Salary and

travelling expenses are included in the award. The host college provides the Visiting Professor with housing facilities.

As an integral part of this program the John Hay Whitney Foundation has established and is maintaining a Registry of Retired Professors in the fields of the Humanities and Social Sciences. The information in the Registry is available without cost to colleges and universities which desire to engage at their own expense the services of retired professors. Requests for information should be addressed to the John Hay Whitney Foundation at 30 Rockefeller Plaza, New York 20, New York.

The Administrative Committee of the Foundation's Division of the Humanities selects the Whitney Visiting Professors and the host colleges. In addition to Dr. Carman, the members of the administrative committee are: Dean William C. DeVane, Yale College; Frederic Ernst, Associate Superintendent, Board of Education, New York City; Charles S. Johnson, President, Fisk University; Marjorie Hope Nicolson, Professor of English, Columbia University; Cyril W. Woolcock, Principal, Hunter College High School.

U. S. Department of State. William O. Brown, for the past five years Chief of the African Branch of the Division of Research for the Near East, South Asia, and Africa, has left the Department to become director of the newly established African Research and Studies Program, Boston University, as of July. In the interim he is traveling on a five-months survey, for the Ford Foundation, of facilities and needs for African research in Europe and Africa.

W. Wendell Cleland will be on leave from the Department during the academic year 1953-54 to serve as acting President of the American University at Cairo.

Dudley Kirk has returned to the Department following a three-months detail to the staff of the President's Commission on Immigration and Naturalization to assist in the preparation of the Commission's Report. He has recently been named Chief of the planning staff in the Office of the Special Assistant for Intelligence, following two years service as Adviser on Sociological Research.

Christopher Tietze has returned to his post in the Office of Intelligence Research, following a three-months field survey during the winter that took him to Europe and to Pakistan, India and Ceylon.

The Committee for the Scientific Study of Religion will hold its fall meeting on November 21 at Harvard University. Scholars who wish to present empirical research in the field (12 minutes allowed) should send 300 word abstracts to Professor Prentiss Pemberton, Andover-Newton Theological School, Newton Centre 59, Massachusetts, by October 1.

The City College of New York. Charles H. Page, Visiting Chairman and Professor, has been appointed as Consulting Editor to Doubleday and Company, Inc., for the development of their publications in Sociology and Anthropology. Professor

Page will return in the fall to Smith College as Professor of Sociology.

Burt W. Aginsky, Associate Professor, is engaged in making a study of "Culture in Port Cities" throughout Europe and Africa, in association with Professor Ethel Aginsky, Department of Sociology and Anthropology, Hunter College. He will return to City College next September.

Robert Bierstedt, Associate Professor at the University of Illinois, has been appointed Professor and Chairman of the Department of Sociology and Anthropology succeeding Professor Page. He will take up his new duties September 1, 1953.

Harry M. Shulman, Associate Professor, was Chairman of the conference on College-Community Relations for Functional Education at Vassar College in April 1953. The Community Service Division of which he is Director, has produced in cooperation with the City College Institute of Film Techniques a teaching film entitled "Step by Step," describing group work with street clubs and gangs.

Melvin Herman, Assistant Professor, will direct an experimental camp for orthopedically handicapped children at Camp Oakhurst, New Jersey, during the summer. The camp is sponsored by the New York Service of Orthopedically Handicapped.

Warren Brown, Assistant Professor, has been appointed a member of the Mayor's Committee on Puerto Rican Affairs by Mayor Vincent Impellitteri, New York.

Alfred P. Parsell, Assistant Professor, will direct the Summer Field Seminar in Social Research this summer. The Seminar will be held in the New York metropolitan area and will be associated with the Yorkville Community Study currently being conducted under the direction of the Cornell Medical School. Robert K. Burns, Jr. will serve as assistant director of the Seminar.

J. R. Champion, Lecturer in Anthropology, has received a grant from the Department of Anthropology at Columbia University to study the process of culture change among the Tarahumara Indians of the Northwest Sierra Madre of Mexico.

Robert K. Burns, Jr., Fellow, has been awarded a research training fellowship by the Social Science Research Council for the purpose of pursuing a year's field study in France. His research will involve primarily a peasant community study in the Maritime Alps of eastern France close to the Italian border, in the region roughly corresponding to the old Provinces of Dauphine and Savoie. The study will begin in August 1953 and continue until early September 1954.

The District of Columbia Sociological Society, the D. C. Psychological Association, and the Washington Linguistics Club have announced the formation of a Joint Committee on Language Behavior. Paul L. Garvin, of the Institute of Languages and Linguistics, Georgetown University, has been named temporary chairman. Ivor Wayne, Bureau of Social Science Research, The American University, is secretary pro-tem.

The Committee's activities will include the planning of joint meetings on problems of common interest to the participating societies, as well as sponsorship of more intensive seminars for groups

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of interested specialists. In addition, the committee will serve as a clearing-house for information on pertinent research projects underway in the Washington, D. C. area.

Further details of the Committee's program may be obtained from its secretary.

Harvard University. In connection with 400th Anniversary of the National University of Mexico, P. A. Sorokin was granted, *in absentia*, among thirteen foreign scientists and scholars, the degree of *Doctor Honoris Causa* by this University. Doctor Lucio Mendieta y Nunez, editor of the *Revista Mexicana de Sociologia* and director of the Mexican Institute of Social Investigations, accepted the degree in behalf of P. A. Sorokin.

University of Kentucky. Ward W. Bauder, Professor of Rural Sociology, resigned February 1 to accept a position at the University of Illinois. C. Paul Marsh, now enrolled in graduate work at Cornell University, has been named Assistant Rural Sociologist, effective July 1.

Joseph H. Jones, graduate student and staff member in the Bureau of Community Service, has accepted an appointment as extension rural sociologist in Louisiana.

The annual Rural Leadership Institute for rural pastors, community leaders, and extension personnel was held on the campus April 14-15. Samuel W. Blizzard of Pennsylvania State College was a featured speaker.

Irwin T. Sanders, Distinguished Professor of Sociology, who has been in Greece for his Sabbatic year, is expected to return to the campus for the fall term.

For the third successive year, a Seminar on Intergroup Relations will be held during the summer session, June 23 through August 1. This seminar is sponsored jointly by the University and the National Conference of Christians and Jews. Gordon W. Lovejoy will be director, and Frances Tierman will be assistant director. The seminar is interracial and carries university credit. Scholarships are available through the state offices of the National Conference of Christians and Jews in southern states.

Rupert Vance of the University of North Carolina was a recent speaker on the Blazer Lecture Series. While on the campus Vance spent some time conferring with the sociology staff and graduate students. Ralph Bunche, Director of the Trusteeship Department in the United Nations, was also a recent Blazer Lecturer.

James W. Gladden, Associate Professor of Sociology, visited the campuses of North Carolina State College, Meredith College, and the Women's College of the University of North Carolina, April 12-19, as the speaker in annual programs in those schools on Courtship and Marriage.

Howard W. Beers, head of the departments of sociology and rural sociology, was a staff member in the Interdenominational Town and Country Ministers' School at the Rural Church Center, Green, Lake Wisconsin. He offered a course on "Community Organization."

Ralph J. Ramsey, field agent in rural sociology,

was elected vice-chairman of the North Central Rural Sociology Committee at the meeting in Chicago on April 2-4.

Mississippi Program in Sociology and Anthropology. The Division of Sociology and Rural Life at State College and the Department of Sociology and Anthropology at the University have received the official approval of the Board of Trustees for cooperative programs in research and graduate instruction. One joint research project is under way and two are being planned. In 1953-54, methods of making the teaching specialties of both campuses available to all graduate students will be experimented with.

Two distinguished lecturers visited the state for a month each under the Liberal Arts Development Program of the University. During March, Ralph Linton, Sterling Professor of Anthropology at Yale University, gave two series of lectures at the University on Culture and Personality and on the Growth of Civilization. While here, Professor Linton received the cabled invitation to the Royal Anthropological Institute of London to give the Huxley Lectures and receive the Huxley Medal in 1954. Dr. Linton conducted a seminar for the staff and graduate students of the two departments on Stratification in Simple and Compound Societies. During April, Ernest W. Burgess, Emeritus Professor of Sociology at the University of Chicago, gave two series of lectures at the University on Courtship and Marriage Adjustment and participated in the annual meeting of the Mississippi Council on Family Relations, for which Dr. Paul Carter of State College was the presiding officer. Dr. Burgess conducted a seminar for the staff and graduate students of both departments on Some Methodological Problems in Sociology. Members of the Social Science Seminar at State College also participated.

The Social Science Research Center at State College has initiated a study of local action in one of three communities selected for intensive study. Alexander Fanelli and William Buchanan are in charge.

Harold Kaufman and Raymond Payne (State College) and Julien Tatum (University) represented the Mississippi Program at the sessions of the Southern Regional Committee on Community Study held in Chattanooga, March 26, 27.

Raymond W. Mack (University) has been awarded a Fellowship in Business for the summer of 1953 by the Foundation for Economic Education. He will spend six weeks with Stockham Valves and Fittings, Birmingham, Alabama.

Morton King, chairman of the University department, is president-elect of the Southern Sociological Society to serve in 1954-55. He has previously served as secretary-treasurer and first vice-president. Dr. King will visit Michigan State College during the summer to teach Minority Groups and Culture and Personality.

University of Missouri. On Saturday, October 17, the Department of Sociology and Anthropology and the Department of Rural Sociology will be hosts to a state-wide meeting of sociologists in the Memorial Student Union. Other sociologists

are also invited to attend. Present plans are to build each of the two sessions around a single paper, and to distribute copies well in advance, to those planning to attend so that the sessions can be devoted entirely to group discussion. The informal committee in charge consists of Chester Alexander, Westminster College; David Carpenter, Washington University; E. G. McCurtain, Drury College; and Toimi Kyllonen, University of Missouri.

Irwin Deutscher has received an SSRC predoctoral research training fellowship. Using Kansas City as his locale, he will make a study of the adjustment of middle-aged married couples whose children have left the home.

William H. Sewell presented the Assembly Lecture sponsored by the Department of Sociology and Anthropology and the Department of Rural Sociology. His subject was, "The Outlook for Social Science Research."

David Riesman was the guest speaker at the spring banquet of Alpha Kappa Delta.

Hubert Bonner is Visiting Professor of Social Psychology during the summer session.

Oberlin College. George E. Simpson will be on sabbatical leave during the first semester of 1953-54, and Professors Richard R. Myers and J. Milton Yinger will be on sabbatical leave during the second semester of 1953-54.

Maurice R. Stein, who is now on the staff at Dartmouth College, has been appointed Instructor in Sociology for 1953-54.

Professor Simpson has received a grant-in-aid from the American Philosophical Society for a field study in Jamaica during the period May, 1953 to January, 1954.

Ohio Valley Sociological Society. The Fifteenth Annual Meeting was held in Columbus, Ohio, April 24 and 25, with the Society for Applied Anthropology. Sections featured interdisciplinary research, research methods, social psychology, race and ethnic relations, industrial sociology, the family, and contributed papers. A luncheon meeting was addressed by Samuel A. Stouffer. The presidential address was delivered at a dinner meeting by Raymond F. Sletto. Carl A. Nissen was chairman of the Committee on Local Arrangements. The president for the coming year is Harold T. Christensen of Purdue University.

Pacific Sociological Society. A meeting of the Northern Division was held April 23 and 24 at Hotel Gearhart on the Oregon coast. Nine research papers were presented in three sessions. An informal program was presented at a dinner, with remarks by S. Frank Miyamoto and Robert E. L. Faris. The arrangements were made by Dr. Miyamoto, vice president of the Northern Division.

Wayne University. Norman D. Humphrey is spending the year in Mexico studying the changes in Mexican scholars who had studied in the United States. This study is financed by a grant from the Social Science Research Council.

Edward C. Jandy is continuing another year as a public relations officer, United States Department of State, in Addis Ababa, Ethiopia.

Donald C. Marsh is president of the Michigan Sociological Society for the year 1952-53; he is also a member of the Co-ordinating Committee on Human Relations, Detroit, Michigan.

James B. Christensen, from Northwestern University, has been substituting for Dr. Humphrey in the anthropology field.

Stephen W. Mamchur has been elected to the Board of Directors of the National Council on Family Relations.

Stephen C. Cappannari taught last summer at San Jose State College, San Jose, California, and at the San Francisco State College, San Francisco.

Washington State College. A Sociological Research Laboratory has been established in the Department of Sociology under the direction of Dr. Julius A. Jahn. This Laboratory is developing a program of Research for the development and application of experimental and statistical principles and methods applied to problems of classification, scaling, enumeration, and prediction in sociology. Research projects are to be planned and carried out with the cooperation of state and local health, welfare, and educational organizations. The Sociological Research Laboratory replaces the Office of the State College Director of the Washington Public Opinion Laboratory.

University of Washington. George A. Lundberg spent several weeks at the University of Hawaii, teaching in the summer session.

Stuart C. Dodd is spending the summer in Europe, visiting universities in several countries, lecturing, and consulting with scholars. He will attend the International Sociological Congress at Liege in August.

Norman S. Hayner has been granted another year of leave from the department to continue his experience as a member of the parole board.

Delbert C. Miller returns to regular teaching in the department after his two years as director of the Airsite Project. Nahum Z. Medalia of the University of Washington will be in charge of the project for the coming year.

The *Review* has learned with regret of the death of Lucille Eaves, Professor Emeritus of Simmons College and Emeritus member of the Society. Dr. Eaves received the Ph.D. in sociology at Columbia University. She is author of a History of California Labor Movement and has contributed to a number of volumes and written many articles and book reviews. She had been a member of the Society since 1910.

Belated news has reached the office, telling of the death in April 1952, of Paul R. Stevick of Joplin Jr. College, Missouri. Dr. Stevick received the Ph.D. in Social Psychology in 1931 at the University of Iowa and was an active member of the society since 1945.

The death of Lea Mandelbaum, of Newark, New Jersey, a student member since 1949, has just been reported. Mrs. Mandelbaum received the B.A. from Rutgers University and was recently a graduate student at Columbia University.

BOOK REVIEWS



Psychoanalysis as Science: The Hixon Lectures on the Scientific Status of Psychoanalysis. Edited by E. PUMPIAN-MINDLIN. Stanford: Stanford University Press, 1952. x, 174 pp. \$4.25.

The five lectures were delivered in the Spring of 1950 at the California Institute of Technology by E. R. Hilgard, L. S. Kubie, and E. Pumpian-Mindlin. All three lecturers show respect for Freud, and are sympathetic to, and defensive of, psychoanalysis. Although realistic in their recognition that most of the present general Freudian theory does not rest on a rigorously scientific basis, they all appear to expect the experimental material produced in the coming years to confirm its principal features. In the present stimulating and illuminating lectures there is a clear presentation of the operations of the analytic process, of the outlines of the Freudian theory and some modern revisions, of selected objective research studies which appear to relate to the theory, and of the prospects of improvement in research methods in this field.

In the judgment of the reviewer, the lecturers reveal, partly between the lines, at least ten major shortcomings of scientific spirit and method which possibly constitute the major barriers to the establishment of secure knowledge in this field.

(1) *Inadequacy of detachment and objectivity.* For one thing, the authority of Freud is a smothering influence. Freud is called (p. 157) ". . . the towering figure in psychoanalysis, whose light shown so brilliantly that he has obscured all others, and, to a certain extent, still does." Furthermore, as in other forms of clinical practice, the commitment of the analyst to a particular form of treatment sometimes interferes with his ability to judge its merits objectively. Kubie confesses (p. 116) that ". . . the good therapist will always be in some measure an ax grinder . . . it is unrealistic to expect perfect scientific detachment." There are, however, certain measures known to the other branches of medical science, which could be used with profit in psychoanalysis. For one thing, no practitioner should be the judge of his own results, and in an experimental study the changes in the experimental group should be measured by an instrument or rated by ex-

pert observers who do not know which persons received the treatment and which did not.

(2) *Distortions resulting from the practice of generalizing on the basis of small and selected samples.* Kubie points out that an analyst works for months or years with only a few patients (p. 114) and adds, "When an analyst discusses some conditions, such as height phobias, for instance, he will base his report on, at most, a mere handful of such patients." He adds that although he has practiced for more than twenty years, ". . . yet there are many conditions which have never come my way." The literature contains many criticisms of the bias in Freud's generalizations, based on his middle-class neurotic Viennese cases. Perhaps the small samples of present-day analysts are biased toward the kind of person who knows of, and is attracted to, Freudian theory.

(3) *Entrapment by inefficient concepts.* Hilgard refers (p. 15) to a study showing low positive intercorrelations among male undergraduate students of the traits of stinginess, orderliness, and obstinacy. This is considered to be evidence in support of the Freudian notion of an "anal character." No connection, however, is established with anal or any other kind of eroticism. It is a widespread practice among adherents to Freudian theory to label all orderliness as "anal" in this fashion, as well as to speak of ordinary stage fright as "castration fear" and status complaint by women as "penis envy"—without establishing the validity of the concepts used. The concepts apparently dominate the beliefs—Kubie asks rhetorically—" . . . why, in spite of the fact that the human race has been divided into male and female for quite a number of years, do we remain *wholly unreconciled* (italics by the reviewer) to the anatomical differences between the two sexes?" The same lecturer, however, recognizes the trick the concepts of id, ego, and superego, play on the psychoanalysts' perceptions, expressing a dislike (p. 105) ". . . of allowing a subtle, anthropomorphizing tendency to inflate these abstractions from the whole personality, endowing each with a spurious independent existence, which then would allow us to indulge ourselves in allegorical imagery and figures of speech about strife *between* them."

(4) *Inadequate formulation of propositions.* Statements cannot be tested scientifically unless they are stated with clarity, and in a form which

allows for the possibility of disproof. This old canon is repeatedly violated in the literature, including the present volume. For support of the notion of the "anal character" a study is cited (p. 16) which found a higher frequency of reported stinginess, and *also of extravagance*, among adults who confessed to a recall of some form of anal eroticism. The literature contains frequent examples of analytic arguments that opposite traits reveal the same underlying condition—if a boy shows dislike for his father it is unconscious sexual jealousy and hatred, if he admires his father it is the same sentiment further disguised.

Vagueness, masquerading as precision, is illustrated by an assertion by Kubie. Referring to an instance in which a boy perceived an analogy between a coal wagon discharging its load, and his own eliminative process, he stated that "*it is mathematically predictable that every unitary symbol must have both an internal and an external point of reference [italics his].*"

(5) *Incautious reliance on "the obvious."* Kubie refers (p. 65) to "experiences in which no interpretation was needed," meaning that the interpretation is obvious to anyone. He illustrates it by an instance of a taxi driver who took a route along Eighty-second Street instead of Eighty-first as he had been told. This is "obviously" seen as an unconscious aversion to odd-numbered streets, resulting from betting losses involving odd numbers. It reminds the reviewer of an interpretation in terms of female sex symbolism of the action of a criminal who attempted an extortion on a victim who lived on Woodlawn Avenue. The analyst took it as a matter hardly worth arguing that Woodlawn was chosen for this unconscious significance. Of the nearby parallel streets, however, the majority have such names as Cottage Grove, Maryland, Greenwood, Kimbark, Kenwood, Lake Park, and the statistical probability of missing one of these female symbols is rather low.

Another illustration of the reliance on "obvious appearance" is given by Kubie who refers (p. 102) to the theoretical stages of psychosexual development in children as "Quite obvious and commonsense descriptions of phases through which every child passes. . . ." The present reviewer has watched several children closely through the years of growing and has failed to find one who passed through these "obvious" phases.

(6) *Improper validation by successful prediction.* Hilgard refers (p. 42) with satisfaction to ". . . the correspondence between predictions according to psychoanalytic theory and what is found, . . ." but these may often be predictions which are consistent with rival theories as well. The reviewer recalls a man who broke out

with a case of hives a few weeks before a planned vacation trip. Three theorists interpreted his condition—one in terms of conflict and guilt, one in terms of fatigue, and one in terms of diet. All three, independently, predicted that the vacation would clear up the condition, as in fact it did. Which theory was confirmed? It requires a more sophisticated technique than this sort of prediction to evaluate the achievements of successful prediction.

(7) *Unwarranted claims of confirmation of theory by successful therapy.* Medical science knows that recovery from a bad cold does not necessarily prove the value of a particular treatment. Kubie correctly warns (p. 88) that although ". . . therapeutic results are interesting they prove nothing." Difficult as it may be in psychoanalytic work, adequate experimental controls will have to be employed before anyone can take comfort from any apparent triumphs of treatment.

(8) *Disregard of relevant research in other fields of knowledge.* Psychoanalysis is essentially an applied aspect of social psychology, yet there is little indication that research in psychology and sociology is considered by psychoanalytic theorists. Experimental studies of repression, for example, reveal some tendency for pleasant material to be remembered more than unpleasant material, but these observations cannot be properly interpreted in isolation from the large and valuable studies in perception and memory in the general literature of psychology and social psychology. In general, psychoanalysis would appear to be in sore need of a better formulation of the nature of consciousness, perception, memory, motivation, and the relation of these to physiological processes, in order to be able to escape from the nineteenth-century conceptions which dominated Freud's fertile imagination.

(9) *Incautious transfer of principles from animal to human behavior.* Studies have shown that puppies frustrated in their early sucking may have a lasting tendency to suck various things. This does not prove that the same process applies to the human, and Hilgard cites evidence (p. 5) to indicate that it probably does not. He continues, however, to refer respectfully to studies of hoarding in starved rats, as if this were of some value to the explanation of human hoarding, still recognizing that final validation must be made on human subjects. Apparently the caution is not taken too seriously, however, for on page 11 he illustrates the mechanism of displacement by reference to an electrically shocked rat biting a rubber doll—the only other object in the cage. All this brings to mind the many misguided attempts to equate the demonstrations of animal-reactions-to-conflict with human neuroses.

(10) *Uncritical use of research based on weak methods.* It is understandable that one who is convinced of anything would be inclined to be tolerant of flaws in any favorable evidence, but psychoanalysis is in exceptional need of strong general skepticism. A few examples from the present lectures, themselves far more critical than most in this field, will illustrate.

To support the theory of regression, Hilgard refers uncritically (p. 9) to the Iowa study of frustration and regression by Barker, Dembo, and Lewin. This study has been severely criticized (see, for example, this *Review*, Vol. 7, pp. 140-141) for a variety of deficiencies of method, and should be used, if at all, with full recognition of these. Similarly, the study of the "authoritarian personality" by Adorno *et al.*, which is cited in support of the concept of displaced aggression, is contradicted by other studies which bear on the same problem, and therefore not reliable as evidence of a scientific character.

Hilgard states (p. 4), "The importance of early childhood is one of the most familiar teachings of psychoanalysis." The reference is to material on the influence of sucking and toilet-training experiences on later personality. He mentions on the following page a study which does not bear out this point of view, but later (p. 14) states, "We have already seen how frustrated sucking may give rise to thumb-sucking." Sewell's recent devastating research study (*American Journal of Sociology*, 58, pp. 150-159) reveals the shallowness of presumptions of this type.

One further example should suffice. Concerning the sex drive, it is said to disappear almost completely in the presence of prolonged life-endangering situations. This is supported by a study of inmates of concentration camps, and Pumpian-Mindlin adds, "There is sufficient extra-analytic evidence to confirm these characteristics of the sexual drive. And there is little controversy in the analytic field about the phenomenology of the sexual drive." The "extra-analytic evidence" referred to should have been enough to cause modification of the claim, for it referred to a study of starvation diets which apparently reduce sex desire. Would other "prolonged life-endangering situations" not involving protein deficiency reduce sex desire? A sophisticated survey of various kinds of dangerous activity—military, criminal, and the like—is needed before the proposition is to be stated in the above form.

Intensive clinical treatment is doubtless here to stay. How much of the distinctive Freudian theory survives is another question. An organization of closed minds can hold to erroneous ideas for an indefinite time, but to the extent that

leading persons in the psychoanalytic movement concern themselves with scientific method there is possibility of progress. Sociology, at the beginning of its scientific stage, beset with an abundance of methodological difficulties, and addicted to a variety of erroneous procedures, can sympathize with psychoanalysis and wish it success in overcoming its difficulties.

ROBERT E. L. FARIS

University of Washington

Social Psychology. By SOLOMON E. ASCH. New York: Prentice-Hall, Inc., 1952. xvi, 646 pp. \$5.50.

Readers of the psychological journals know Solomon Asch as a Gestalt psychologist concerned with the importance of the social world for the understanding of behavior, as a clever and persistent experimenter, and as a man with a shrewd eye for the defects of theory and design and for the selection of the important from among available ideas. This book confirms all these judgments. It adds evidence that Asch can maintain a clean, vigorous writing style over the long stretch and that his probing and sifting has extended to all manner of social psychological concerns. But the greatest surprise of this book, and possibly its greatest contribution, is its return to the problems that were central in social psychology twenty or thirty years ago and its ability, in giving them new life, to make a strong case for their continued importance today.

It is rare when the author of a recent text in social psychology returns again to a serious discussion of the plasticity of human biology, of the seeking, exploring character of human action, of the conditions of volition in behavior, or of the nature of self control. Such topics do, in truth, find their way into the pages of most texts. What is distinctive about Asch is his treatment of them as controversial centers for the organization of his work.

The chapter headings and general purpose that embrace all this probing and excitement are common enough, except for their Gestalt flavor. We begin with a survey of the processes given in human biology for experiencing the environment, and of the application of these processes to the special case of experiencing the environment provided by other humans. Then follow consideration of the development of a personal identity in the individual and of a distinctive culture in the group, and the conditions within which the predispositions of the individual are modified in interaction. The major focus is that of the psychologist using the fact of social experience to help explain the dynamics of the individual, rather than of the sociologist

forced to state certain assumptions about individual behavior in order to explain some pattern of interaction. There are exceptions to this judgment, but it represents the central tendency.

One of the most significant contributions of the Gestalt movement to all thinking about behavior, and about the elementary discussions in this book, is its insistence that organisms behave toward the world as they experience it; that one critical job for theory construction is the provision of a set of concepts that will enable the description of the world as experienced. This compares with G. H. Mead's theme of the individual "carving out" his own world and Thomas' emphasis on understanding behavior in terms of the actor's "definition of the situation." Neither Mead nor Thomas provided us with a set of categories suitable for representing such definitions of situations. This is something the Gestaltists began. They also worked with concepts compatible with the sociological observation that the world is experienced in wholes; concepts that promised to provide a basis for deriving such social phenomena as the "strain toward consistency" among norms, and the experience of the group as an entity different from the sum of its parts. Together with the recent work of D. O. Hebb and Kurt Lewin, Asch provides us with a basis for reviewing the present state of this line of explanation. Unfortunately, unlike Lewin, Asch does not attempt to provide a systematic set of concepts especially suited for the behavioral description of social phenomena.

It must be confessed that Asch, like most of us when we work with data from another specialty, is not as sophisticated about social phenomena as many sociological readers will desire. However, he does know an important problem when he finds one. He deserves a round of thanks for forcing attention to such questions as: Can sociologists develop theories without the explicit use of behavioral processes as independent and dependent variables? Is the much hallowed interpretation of empathy, as a matter of making correct analogies with one's own experience, a valid interpretation? Can interpersonal influence be discussed meaningfully without specification of the situation in which it occurs? Does the fact of cultural relativism preclude the development of validated universal principles of social behavior? Are current reinforcement theories of behavior adequate for the explanation of what he calls the experience of "obligation" and what Cooley would have phrased as the "sentiments?" Is the ego fundamentally ego-centered? Are variations in the strength of the social bond explained by variations in the "private profit" gained by the individuals involved? Is it the scientist or his subjects who really perceive the world through highly simplified stereotypes?

In giving new life to these old questions, Asch is not always just to his critics, not always careful about presenting the strongest arguments of the opposition, and often incautious in his interpretations of data. For the reader willing to accept his purposes and scholarly background as they are, there is a rich reward of stimulation.

G. E. SWANSON

University of Michigan

Personality Measurement. By LEONARD W. FERGUSON. New York: McGraw-Hill Book Company, 1952. xv, 457 pp. \$6.00.

Most sociologists and psychologists who read this book will probably find it very difficult to get beyond the first chapter, which contains some of the most banal and naive observations and writing which this reviewer has seen in recent years in a textbook. For example, as an illustration of "the effect of group on individual," a Kinsey finding of a linear relationship among rural boys between amount of education and frequency of sexual contact with animals is cited. Correlations of .23 and .33 between diameter of areola (pigmented ring around the nipple) and interviewer's rating of degree of "maternal feeling" are advanced as illustrations of "constitutional determinants" of personality; and as an example of the "effect of the individual on the group," Marriott's *Maria, The Potter of San Idelfonso* is recommended "... as a must for anyone interested in the effects which the personality of one individual can have upon the characteristics and way of life of a cultural group."

Yet, if the reader can find the courage to persevere beyond this flappedoodle, he will find good payment for his efforts. The very next chapter (Chapter 2), in fact, gives an exemplary discussion of the Strong Vocational Interest Test. The strong points of this book are what one would expect from a successful applied and industrial psychologist—practicality, and a realistic evaluation of tests and assessment devices.

The weaknesses, outside of the near-fatal first chapter, have to do mostly with matters of structure and with editorial aspects. The tables, for example, are overly complicated and difficult to interpret, with the necessary information for headings, captions, etc., usually missing. Names are constantly mentioned in the text without reference citations, and a goodly number are presented without any listing in the bibliography at all. The writing, too, is often awkward and cumbersome and imposes a strain on the reader.

What this book needs, in the present reviewer's opinion, is a re-write and thorough overhauling under expert editorial guidance. It has many of the elements of a good book—broad

coverage, careful scholarship, and critical thinking—but the writing, organization, and general presentation is unlikely to attract the audience which the book's essential merit deserves.

HARRISON G. GOUGH

University of California, Berkeley

Innovation: The Basis of Cultural Change. By H. G. BARNETT. New York: McGraw-Hill Book Company, Inc., 1953. xi, 462 pp. \$6.50.

Thirteen years ago in an article entitled "Cultural Processes" (*American Anthropologist*: 42 [1940] 21-48) Barnett gave added impetus to the then quickening interest in theoretical understanding of culture change. Drawing upon his field work among Indians of northwestern California, he outlined a fresh and suggestive approach to problems of cultural dynamics in contact situations generally. His impatience with the older anthropology's "diffuseness of method and . . . lack of instrumental concepts" focussed particularly upon the inconclusive and conditional propositions which theretofore had been advanced to explain change. "Even diffusion, our most illuminating concept," he noted, "gives only an uneasy satisfaction and raises as many problems as it solves. We know almost nothing of its mechanisms, its patterns, or its limitations" (p. 21). With rare modesty he called his article merely "a small beginning at the right end of the problem of cultural dynamics," the right end being simply the discovery of uniformities and regularities among concrete forms of adaptive behavior in specific situations. In respect of changes resulting from contacts between peoples of differing cultures—the subject of that article—his approach insisted upon careful examination of the conditions and processes surrounding particular cultural innovations. This procedure foreshadowed what Firth later called the micro-sociological view of small-unit behavior, and stands in sharp contrast to broad overviews of the matter such as, for instance, Toynbee's encounters between civilizations.

A broader comparative application of the same principle of concentrated observation, both within and between cultures, resting upon a social science base wider than that of anthropology alone, has produced a rich harvest in *Innovation*, a book which must rank at once as both the most comprehensive study of a specific cultural process yet available, and the most important general work on cultural change in this generation. This is a work which shows no trace of having been done under the dangling sword of a deadline; nor does it compromise with scholarly integrity by catering to the pedagogical marketplace. In its scope and organization, its tightly-textured interdisciplinary relevance, and

especially in the steady tenor of its sustained analysis, this is a book which represents both an original contribution and a mature achievement. LaPiere in his editor's introduction rightly calls it "a synthesis of our present knowledge and a base from which new investigations can take off." Students of cultural change in any of its aspects will have to reckon from this point onward with the co-ordinated view of culture growth which Barnett has here provided.

The major purpose of this study is "to formulate a general theory of the nature of innovation and to analyze the conditions for, and the immediate social consequences of, the appearance of novel ideas" (p. 1). Innovation itself is conceived to be ultimately a mental phenomena, which accounts for the book's emphasis on psychological universals in individual behavior—an emphasis that is further clarified and justified in an appendix "On Things." But the social and cultural conditions surrounding the appearance of new ideas are never slighted. Innovation consists in a fusion, or linkage, of previously unconnected elements—thoughts, behavior, or things—in a qualitatively new and distinct whole. Such a fusion may or may not be visibly manifest, but to become socially relevant the novel configuration must at some point occur in a mind or minds. The scope of these views is obvious: they comprehend differential behavior of any kind or degree wherever found. To cut a trail through such forests of potential fact, a trail which has some hope of an ending, Barnett divides his argument into four main areas of exposition and analysis: (1) *The Setting*, in which are briefly considered peoples and cultures of the world in their innovative aspects and potentialities; (2) *The Incentives to Innovation*, in which motivating factors of personal wants and desires, as mediated by culture, are examined and classified; (3) *Innovative Processes*, in which, with the aid of a few simple diagrams of relationship, the union of ideas in new configurations is subjected to long and detailed analysis; and (4) *Acceptance or Rejection*, in which personal and cultural conditions surrounding the selection of innovations are assessed.

No summary of content, however, can suffice in recording some of the subtler virtues of this volume, such as the step-by-step presentation of its developing argument, the polyphonic integration of its anthropological, sociological, and psychological concepts, and the variety and wealth of its case materials on innovation. To his earlier data have been added many other specific examples gathered at first hand—among other northwest Indian groups, the Indian Shaker cult, and Micronesians of Palau especially. And he draws widely and often from the unprecedentedly rich history of innovation in modern Euro-

American culture. More sparing but no less pertinent use is made of culture contact materials gathered by others in Africa and Oceania. In terms of sociological theory, one also must note the many-faceted treatment of problems of relationship among persons, ideas, and things; in fact, this book devotes more attention to this subject in passing than do most texts supposedly devoted to it.

Evaluation of the psychological concepts used here must be left to others who have more competence in that field than the reviewer. At least it may be said that they make sense. The absence of reference to learning theory, however, in discussion of "ramifications" and "compound processes" of innovation, as well as in acceptance and rejection, may surprise some readers. It must also be noted that a summary statement of "a general theory of innovation" is nowhere given us: the general theory remains implicit. This may well be a virtue rather than a fault in our present state of knowledge. Certainly the corpus of lesser theories which it contains can be used in part or whole by a wider circle of social scientists than might have been the case had the author set his sights on a "system." If any criticism of the book is called for, it might first be directed toward the publisher rather than the author. Here is an important book in social science; and, *mirabile dictu*, a well-written one. But in typography, stock and format—in fact, in everything but price—it recalls the unlamented austerity models of war-time publishing.

STEPHEN W. REED

Yale University

Culture: A Critical Review of Concepts and Definitions. By A. L. KROEBER and CLYDE KLUCKHOHN, with the assistance of WAYNE UNTEREINER and appendices by ALFRED G. MEYER. Papers of the Peabody Museum of American Archeology and Ethnology. Harvard University, Vol. xlvii—No. 1. Cambridge, Massachusetts, 1952. viii, 228 pp. \$5.25.

In the last two decades, a concern with the cultural dimension of human behavior has been invading one specialized field after another: clinical psychology, psychiatry, criminology, social work, education, international relations and many others. In the author's words, culture has become "one of the key notions of contemporary American thought," and tacking the qualifying remark "in our culture" to all generalizations has become one of the rules of intellectual etiquette. This rapid proliferation of ideas and attitudes radiating from the central concept "culture" has produced an extremely wide range of meanings. Professors Kroeber and Kluckhohn

feel that the time has come for "stock-taking, for a comparison of notes . . ."

In carrying out this design they have succeeded in blending the formula of a documentary source-book with an essay on the history of ideas. The book is remarkably free from personal bias. In surveying hundreds of written statements regarding the term under consideration, the two authors at no time imply the existence of any "true" or "correct" definition. Instead their aim is to produce a conscious awareness of the possible range of variation and to provide a historical perspective on the many ideological contexts in terms of which the concept "culture" has been used. Their coverage includes historians (from Herodotus to Toynbee), philosophers (Kant, Herder, Hegel, Nietzsche, Cassirer, Ortega y Gasset, et al.), literary men (Matthew Arnold, T. S. Eliot), psychoanalytical writers (Freud, R  heim, Kardiner) and, of course, the complete gallery of major and minor figures in anthropology and sociology.

In the book the many definitions of culture are grouped on the basis of the comparative emphasis which their authors place on either descriptive (content-enumerative) or historical, normative, psychological, configurational, genetic and other elements. This is followed by a chapter offering a selection of significant statements about culture, e.g. its nature, its components, its distinctive properties, its relationship with individual psychology, with language and with society. In making up this selection, the authors have treated the concept of culture as a kind of a projective test stimulus capable of eliciting forceful personal value-judgments and theoretical disquisitions. This has resulted in a particularly rich thesaurus of ideas.

The anthological appearance of the volume should not mislead one to minimize the original character of the two authors' contribution. Their statements interspersed among other men's writings or relegated to the end of the book could easily be rearranged into an independent essay on the science of culture. Their argument favors the operational view of culture as a reality-level *sui generis* (without any ontological implications), not reducible to individual psychology or to a statistical portrayal of the distribution of patterns of thought and behavior.

A. L. Kroeber and C. Kluckhohn are aware of the diffident attitude shown by some psychologists and social scientists regarding the extreme claims made by the users of the concept of culture. They reassure them by saying that "anthropologists do not claim that culture provides a complete explanation of human behavior, merely that there is a cultural element in most human behavior, and that certain things in be-

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The book concludes with two useful essays by Alfred G. Meyer on the concept of the culture in Germany and Russia.

JOSEPH BRAM

New York University

The Quest for Community: A Study in the Ethics of Freedom and Order. By ROBERT A. NISBET. New York: Oxford University Press, 1953. ix, 303 pp. \$5.00.

The argument of this book is stated in Part I. Modern society is characterized by social disorganization and personal insecurity, if we may judge by selected scientific studies (Durkheim, Mayo, Horney) and by the literature of alienation (Toynbee, Eliot, Berdyaev, *et al*). The essential cause of our insecurity is the dislocation of kinship, church, and other local groups by the centralizing State. In the face of this *anomie* the key 19th-century ideas of individualism, secularism and progress are on the defensive. The quest for community is the author's concern for the intimate social solidarity and moral certainty associated with small groups.

Part II reviews the post-medieval era. Nisbet interprets the basic trend in social organization as the decline of community and the rise of the omnipotent State. "The single most decisive influence upon Western social organization has been the development of the centralized territorial State" (p. 98). The history of the relevant political ideas associated with this process is then summarized. The culmination of the modern era is totalitarianism, defined as a mass of isolated persons who find their community mainly in the central State, once the intermediate local and kinship groups have been undermined. To Nisbet this signifies the "annihilation of individuality" (p. 201).

Part III asserts that freedom inheres in autonomous local groups. "Freedom thrives in cultural diversity, in local and regional differentiation, in associative pluralism, and, above all, in the diversification of power" (p. 265). Solution of the contemporary problem of order requires a renewal of small-group contexts for many social relationships. How this can be accomplished the author fails to say. He recognizes, however, that we cannot turn back the clock.

This book is essentially a conservative ideological essay. It stresses the social values of localism and familism; it carefully elucidates the "good" (i.e., community-making) aspects of war. Yet it unwittingly documents some leading arguments of the Left. That anxiety and disorganization and a literature of decay are 20th-

century characteristics is true. But they are specific features of "old regime" societies, like the Western nations today, preoccupied with war and the imminent dissolution of their world. Nisbet's conception of the modern mass revolutions abroad is unfortunately patterned after Orwell's 1984. A more Olympian view would reveal that there is confidence and reconstruction in the present era as well as pessimism and destructiveness.

The analysis of family roles and small-group values is brilliant. In its broader aspects the reasoning may seem overdrawn to some readers. Nineteenth century "individualism" has something of the straw man about it, and so too has the so-called twentieth century "atomizing" of the masses. The theory of the primacy of the State in modern social change is surely one-sided. Is it not more useful to conceive of change in terms of a system of elements wherein no single factor leads or lags except for purposes of analysis? Nisbet come perilously close to a one-factor theory of change.

His identification of freedom with cultural diversity and decentralized groups is philosophically legitimate but sociologically parochial. A sociological definition of freedom valid for any social system would run more like this: freedom is a subjective feeling of personal well-being which results from the objective fact of living in an effectively functioning society. A society functions effectively to the degree that its social structure is integrated, that it successfully meets its problems of internal and external change, that it socializes new members, satisfies or reconciles their needs and expectations, etc. The point is that a number of concrete patterns of societal organization can meet this abstract definition of freedom. It is entirely possible that among them is Nisbet's bogey, the totalitarian community—once the latter has been stabilized or routinized. In transition eras, societies fulfill their functional requirements less effectively; hence their members feel they have less freedom.

Nisbet sees clearly the malintegration between primary groups and large-scale organization, and the resulting primary-group *anomie*. He fails to consider those sources of personal insecurity which originate in large-scale organization, e.g., the overemphasis on competitive upward mobility, the business cycle and overproduction, institutional pressures toward war and imperialism, the persistence of discrimination against minorities, management-labor conflict. Solution of these problems will require more State action, probably nothing less than comprehensive central planning, which in turn may provide a framework for approaching the small-group problems raised by Nisbet.

The Quest for Community gives us an in-

cisive analysis of certain lesser aspects of the present crisis, but no cure.

Since sociological writing is often criticized for incomprehensibility, it is a pleasure to note how exceptionally well written this book is.

ARTHUR K. DAVIS

Columbia University

Essays on the Sociology of Knowledge. By KARL MANNHEIM (Edited by PAUL KECSKEMETI). New York: Oxford University Press, 1952. viii, 327 pp. \$6.00.

This volume contains six monographic essays written by Karl Mannheim from 1923 to 1930, when he was a brilliant and already mature young scholar in pre-Nazi Germany. The essays have now been lucidly translated and carefully edited by Paul Kecskemeti, who has also written an excellent introduction. In this introduction there is a description of the social and intellectual backgrounds of Mannheim's ideas. The social background was the moral disillusionment and utopianism of post-World War I Germany. The four main components of the immediate intellectual background were Marxism, the phenomenology of Husserl and Scheler, the anti-positivism of Dilthey and Max Weber, and the structural approach of "historicism" in the cultural sciences and of Gestaltism in psychology. The introduction also presents a detailed tracing out of the evolution of Mannheim's ideas in interaction with these social and intellectual forces, and a brief but useful critique. Kecskemeti's somewhat philosophical approach, in this critique, to the crucial problem of relativism and absolutism in knowledge can be profitably supplemented by the more sociological analysis of Talcott Parsons (*The Social System*, Ch. VIII, Belief Systems and the Social System: The Problem of the "Role of Ideas") and Robert K. Merton (*Social Theory and Social Structure*, Ch. IX, Karl Mannheim and the Sociology of Knowledge).

In the first essay, "On the Interpretation of 'Weltanschauung,'" Mannheim answers the question: How do we know and explain cultural phenomena? In reaction against intellectual positivism and atomism, Mannheim declares the independence of "the logic of the cultural sciences which we shall have one day" (p. 35) and the need for a structural or organic concept of system in that logic. Mannheim's question in this essay has recently been very systematically and extensively treated by F. Znaniecki in his monumental *Cultural Sciences, Their Origin and Development* which likewise stresses the concepts of "meaningfulness" and "system."

In the second and third essays, Mannheim

deals intensively with the problem of relativism and absolutism in knowledge. In the second, "Historicism," his solution for this ancient dilemma of thought is that Reason or Truth must be seen as a dynamic, evolving totality rather than as a static, supra-temporal absolute. The third essay, "The Problem of a Sociology of Knowledge," is Mannheim's first statement of the more essentially sociological theory which he later enlarged and developed in great detail in his book, *Ideology and Utopia*.

The theme of the fourth essay, "Competition as a Cultural Phenomenon," is, in Mannheim's own words (p. 198): "Different interpretations of the world for the most part correspond to the particular positions the various groups occupy in their struggle for power." In this struggle, the process of *polarization* creates a dialectic of ideas from which emerge new intellectual syntheses.

The fifth essay, "On the Nature of Economic Ambition and Its Significance for the Social Education of Man," is a pioneer exploration of the "culture and personality" field as it has come to be called since Mannheim's time. Here Mannheim discusses very concretely as well as analytically how social structure defines "success" differently for various groups and how social position molds individual personality and motives, directs ambition, and determines individual life-plans and life-chances. Max Weber's influence is especially notable in this essay.

The last essay, "The Problem of Generations," analyzes the creation by social structure and social change of socially and historically differentiated age-groups and their associated intellectual and moral conceptions. Recent social science has treated only one small aspect of this "problem of generations" in various studies of the sociology of adolescence.

This is but a synopsis of the main points of exceedingly rich material. "Our view of life," as Mannheim says (p. 84), "has already become thoroughly sociological," and he has been a creative leader in the transformation of our thought. Every page of this work still rewards close study. These essays are all the more pleasurable to read because they draw on history, philosophy, literature, art, indeed, all of culture for insight and example. We learn, therefore, about many particular substantive matters as well as about the general sociological problems that are always Mannheim's primary concern. There is here, for example, wonderful intellectual history, especially about Germany in the nineteenth and twentieth centuries.

We can look forward to one more volume of Mannheim's work. The next collection will include two essays from the period covered by the present volume and several others from the period beginning with his emigration from Nazi

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Germany to England in 1933 and ending with his untimely death in 1947. We who are Karl Mannheim's fellow-workers and successors in sociology are fortunate that he has had appreciative friends to make his classic work available to all.

BERNARD BARBER

Barnard College, Columbia University

Origins of American Scientists. By R. H. KNAPP and H. B. GOODRICH. Chicago: University of Chicago Press, 1952. xiv, 450 pp. \$7.50.

This big book is far and away the best study we have of the educational and social origins of American natural scientists. Indeed, it is an outstandingly good example of social research in general. It is based on a very large amount of data collected by statistical analysis, case study, and questionnaire. The analysis is concerned with fundamental sociological problems and yet is careful and modest. The basic empirical finding, among many significant ones, is that the colleges which are *relatively* most productive of natural scientists are those small liberal arts colleges in the Middle and Far West, of secularized Protestant background, whose students come from small town or rural communities without diversified occupational opportunities, and where at least a few of the teachers are energetic, demanding, and warm enough personalities to create emulation and discipleship in some of their students.

Especially since this is a short review, it needs to be pointed out that the findings of this book do not pertain only to the sociology of science, as the title may imply. The sociologist interested in social stratification will discover valuable information about amounts, types, and structural sources of social mobility. The sociologist of occupations and professions will learn about changing career patterns in the United States and about social structural determinants of occupational choice. The sociologist of religion will find new materials bearing on the influence of Protestantism and Catholicism on productivity in science. The social psychologist interested in socialization will read about the molding of motivation and aspiration that goes on continually in American colleges. And, since the findings of the book have basic implications for American educational and social policy, high Government officials, heads of philanthropic foundations, and college presidents can read it with great profit.

Finally, it should be mentioned that this study of the origins of American scientists was made by a psychologist and biologist under the direction of an interdisciplinary committee of the faculty of Wesleyan University (Connecticut). It is, therefore, itself an example of the scientific

contribution made by the American liberal arts colleges of "broad intellectual interest" intensively studied here.

BERNARD BARBER

Barnard College, Columbia University

The World and the West. By ARNOLD J. TOYNBEE. New York: Oxford University Press, 1953. vii, 99 pp. \$2.00.

History, Civilization and Culture: An Introduction to the Historical and Social Philosophy of Pitirim A. Sorokin. By F. R. COWELL. Boston: The Beacon Press, 1952. xii, 259 pp. \$4.50.

Sorokin and Toynbee persist in "bobbing up" together in interesting fashion as authors of books which call for some serious attention from students of the history of civilization and sociology. Indeed, their affinity goes rather further than the chronological synchronism of their published works. They may be fairly described as, in a way, the Siamese-twins of anti-intellectualism (or at least anti-empiricism), neo-Augustinianism, and historical pessimism. Sorokin, at least, glories in this sort of characterization, for he and his publishers once wrote me asking my consent to quote it in an advertisement of one of his books.

The larger works of these two authors, Sorokin's *Social and Cultural Dynamics*, and the first six volumes of Toynbee's *A Study of History*, appeared during the 1930's. Both authors summarized their views in books published in 1948: Sorokin's *Reconstruction of Humanity* (he had already provided a summary in his *The Crisis of Our Age*, 1941, and *Society, Culture and Personality*, 1947), and Toynbee's *Civilization on Trial*, which I had the honor to review jointly in the ASR, August, 1948.

Both authors have been favored with scholarly Boswells who have epitomized their main ideas and conclusions. This service was performed for Toynbee by an English historian, D. C. Somervell, who brought out an abridgement of *A Study of History* in 1947 which was widely praised and did much to create a veritable Toynbee cult. Now, another English historian, F. R. Cowell, has epitomized Sorokin's *Social and Cultural Dynamics*, and *Society, Culture and Personality* in a brief book of about 250 pages (the Somervell abridgement ran to 617 pages). Whereas the Somervell project was mainly an abridgement of Toynbee's own book and language, Cowell's achievement is an epitome presented as the work of another person summarizing and appraising (in highly laudatory fashion) the views of Sorokin. Toynbee's current book under review here is a brief forecast and anticipation

Caused position reviewed (cf ASR, 12, p. 481.)

of some of the topics which will be covered in greater detail in Volumes VII-X of his *A Study of History*, to the completion of which he is now devoting his time.

There is no space for any summary of the fundamental concepts of Sorokin and Toynbee which underlie the contents of the two books here reviewed. I have summarized them briefly in my *Historical Sociology* (pp. 105-115), and both authors are treated in some detail in chapters of *An Introduction to the History of Sociology*. In addition, Pieter Geyl presented a masterly critique of Toynbee's *A Study of History* in the *Journal of the History of Ideas*, January, 1948.

Toynbee's views, as expressed in his *The World and the West*, appear to the reviewer to be, on the whole, rather more realistic and sounder than some of those which buttressed his larger philosophy of history. They may be summarized about as follows:

The clash of the cultures (and the potential clash of arms) of the West and East constitutes the chief world problem in the second half of the twentieth century. It must be solved, or at least kept in pacific restraint, if civilization is not to perish. For the most part, Toynbee lists Soviet (and Tsarist) Russia as a sector of Eastern culture.

Since 1500, at least, the West has been the great aggressor in world politics under the guise of colonialism, modern imperialism, and a modicum of religious and educational missionary enterprise. It has been successful because of the superiority of Western technology, especially since 1750.

The civilization of the East has not been easily brought into a state of harmony with Western culture because the East has only borrowed and accepted mainly one segment, albeit the most important, of Western achievements, namely, our superior science and technology—our advanced material culture. This it did partly because a strange technology was less objectionable than a strange religion (Christianity), and partly in self-defense against the military and economic invasion of the East by the West.

Soviet Communism has made a greater appeal to the peoples of the East than has Western capitalism and democracy because it offers them an ideology as well as a technology. Another important factor is that Communism encourages rather than seeks to suppress the revolt of the colored world against its erstwhile white masters.

What is the solution? One may be found in the fact that, once Western technology is accepted, it is hard for the East to prevent the gradual adoption of other aspects of Western culture. Gandhi understood this trend and sought to frustrate and suppress it. But, more im-

portant, according to Toynbee, is the need for the West to supplement its technological contribution to the East by creating a new spiritual religion (an Anglican revival) which will weld the whole world together in an attitude of human fraternity.

There is little space for comment upon these ideas. Perhaps the best means of mitigating the clash between West and East is for the West to recognize the nature and reality of the revolt of the colored world against Western imperialism, let it run its course, and readjust to the results. It might also help if the West could learn that, even though the East has tended to welcome our technological contribution, this is no reason why it should and must accept with equal eagerness our non-material culture. It is not improbable that the West has a great deal to learn from the East in the latter field.

Mr. Cowell's epitome of Sorokin's philosophy of history and theories of cultural development is an able and faithful summary, more clear and elementary than the treatment in Sorokin's voluminous writings. The first portion presents Sorokin's well-known theory of the recurring fluctuations of cultures. The two basic cultural types are the ideational and the sensate. The former is founded on faith and mysticism and stresses the role of great men, often priests and prophets. The sensate type of culture is based on empiricism, science and technology and is concerned chiefly with the rôle and lot of the masses. Historic cultures have fluctuated from the ideational to sensate, with intervening periods of idealistic culture, a compromise between the ideational and sensate which usually brings a strong stimulus to scholarly, artistic and material activity. Sorokin fervently eulogizes ideational cultures and historic periods and blisteringly condemns the sensate. Most of our evils and troubles today arise from the fact that we are bogged down in the terminal stages of a decadent sensate cultural era. Sorokin offers no adequate explanation of why these eternally recurring fluctuations take place, other than that they arise from forces *within* each culture and not from stimuli from without.

In the remainder of the book, Mr. Cowell applies these theories of the types and fluctuations of cultures to the historic trends in the development of art, music, literature, philosophy, science, ethics, law, social relationships, politics, economics, war, revolution, personality and behavior problems, and the nature of cultural development and social change. The evidence of erudition present in the original volumes is maintained and many charts and graphs are included. These are, as in the original, at times illuminating and valid, while in other places they compare the incomparable and measure the immeasurable.

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The book closes with a prediction of gloom and doom for decades to come as mankind hopelessly plods its weary way through ever greater demoralization and decadence until our sensate period ends and salvation appears with the arrival of an ideational or idealistic age. How desperately Sorokin and Cowell clutch at any straw which appears to them to constitute evidence of impending ideational purity and beauty penetrating the murky clouds of sensate degeneracy can be illustrated by the emphasis they put on the Korean war viewed as an impressive symptom of the triumph of high moral values over selfish materialism and political expediency (p. 247).

There is little or nothing new in the Cowell epitome to anyone who has read Sorokin's larger volumes. But it is a very convenient and reliable short-cut for beginners in sociology or for non-sociologists who wish to obtain as speedily and easily as possible a good general idea of Sorokin's basic concepts and methods. On the whole, the epitome is clear and well-written although at times the readers will be compelled to wrestle with needless semantic monstrosities as, for example, when the social philosophy of Liberalism emerges on page 115 under the guise of "Collectivistic Sociological Singularism."

The value of the epitome, as of Sorokin's original works in this field (aside from the vast body of incidental knowledge compiled and presented), depends largely upon the degree of validity which can be assigned to Sorokin's general and fundamental theories of cultural types and fluctuations throughout history. On this matter, even expert and objective appraisals may well differ widely. The reviewer, with some considerable knowledge of the facts involved in the history of civilization and the processes of social change, is unable to concede any validity whatever to Sorokin's classification of cultures or his theory of social and cultural change. The basic types of cultures can be more accurately and illuminatingly characterized, and far more substantial and intelligible interpretations of the course of human history and cultural evolution can and have been propounded. This does not, however, preclude the cheerful concession by the reviewer that Sorokin is easily the most erudite living social scientist. It is just too bad that his vast learning has been distorted by extreme personal subjectivity.

Any man is surely entitled to his personal religious convictions, his mystical experiences, and his ecstatic visions. But to synthesize, integrate and project these highly personal and purely subjective reactions into a pretentious philosophy of history and then offer the latter as a valid explanation of cultural evolution and human ex-

perience surely approaches the borders of phantasy. If Sorokin's philosophy of history is a fair sample of the ideational conceptions which are to rescue mankind from the foul abyss of sensate existence, then we shall be likely to step from the frying-pan into the fire if such an ideational epoch should ultimately dawn on humanity.

HARRY ELMER BARNES

Cooperstown, N. Y.

Titoism and the Cominform. By ADAM B. ULAM. Cambridge: Harvard University Press, 1952. viii, 243 pp. \$4.00.

This is an account of events concerning the sovietization of Eastern Europe. Two phases of this process are presented: the pre-Cominform, and the post-Cominform, differing in the form of Communist control and in the degree of economic and political dependence upon Moscow. In the course of a process of regimentation which leads to the unconditional subordination to the will of the Kremlin, there were some leaders in all Communist parties of Eastern Europe who had some difficulty in adapting themselves to the realities of Soviet control. These people showed a certain lack of understanding of some fundamental principles of Bolshevik rule of satellites, such as the "liberating role" of the Red Army, and the leadership of the Soviet Union in the world revolutionary movement ("the base of the Soviet world revolution"), to which objective all local national interests have to be subordinated. It was to have an over-all instrument in the application of such policy that the Cominform was organized. The Bolsheviks have enforced this policy of unconditional submission also by splitting the leaderships of the satellite Communist parties into rival factions, by eliminating from the leadership people who have considerable local following and support, and by placing Russian experts as advisers in all key positions in the administrations of these countries.

While in all other Eastern European satellites this policy required only a few purges of top Communist leaders, in Yugoslavia the process of adaptation was more difficult and resulted in the expulsion of the Yugoslav Communist Party from the Cominform. The leaders of the Yugoslav Communist Party had accepted unreservedly the fundamental principles on which the relations between the Soviet Union and the satellites are based. They had, however, shown a great deal of naïveté concerning the actual application of these principles and their ritual. Since they had so clearly demonstrated in the course of the war that the interests of

the Soviet Union and not those of Yugoslavia were of primary importance to them as true Communists, they childishly believed that this demonstration of blind faith plus their outstanding achievements in fast sovietization of the country gave them the right to exhibit some local independence of action. They also thought that they could overlook some of the ritual that has been established in the relations between the Communist Party of the Soviet Union and other Communist parties, such as, the attitude of humbleness, of reverence and of gratitude toward the Soviet Union and its representatives, even when these function as mere intelligence agents in satellite lands. The punishment for the slightest show of independence or disregard of the ritual is, in outstanding cases, death. In Yugoslavia, however, for reasons of an historical nature, the Russians had not advanced sufficiently in their policy of splitting the leadership and placing their own agents in all key positions, particularly in the police and in the armed forces. Therefore unable to liquidate Tito and his clique, they had to expel the whole Party from the Cominform.

"Titoism" is therefore not an ideology, states the author, but a name covering a variety of reasons why a number of Communist leaders were unable to adapt themselves to the realities of absolute control of the Soviet Union. It sounds therefore like wishful thinking when the author, in the last part of the book, claims that "Titoism" might eventually develop into an ideology which will differ from Bolshevism. The structure of Communist Yugoslavia closely follows that of the Soviet Union. This structure must be perpetuated as a means of self-perpetuation in power of the present ruling faction, as the author himself admits. How then can "Titoism" develop into an ideology essentially different from Bolshevism, as long as the main function of the ideology of such a ruling group is to maintain the existing monolithic power-structure? The question of whether or not a certain degree of local autonomy of Communist parties is in the interest of Soviet world revolution, is not sufficient in itself to form the content of an ideology if all other features of the ideology are essentially the same as those of Bolshevism. But if "Titoism" fails to develop into an ideology essentially different from Bolshevism in a country whose masses are anti-Communist and whose peoples are torn by national rivalries and by conflicting political orientations, can the West rely on Tito's Yugoslavia as an ally in case of war? Perhaps these should be the "grounds upon which the West should determine its attitude toward Tito."

D. A. TOMASIC

Indiana University

Kuomintang and Chinese Communist Elites.

By ROBERT C. NORTH. Stanford: Stanford University Press (Hoover Institute Studies, Series B: Elite Studies, No. 8), 1952. vii, 130 pp. \$1.75.

One of a series on the elites of major countries, this is an urbane and witty study of great value to all those interested in leadership, the development of contemporary China, or the extra-Russian activities of the Communist Party. The scholarship is outstanding, the prose style excellent. There is, however, no index.

Following an analysis of the collapse of the imperial inner circle, the author discusses the development of the Kuomintang; the emergence of Communist Party leadership; characteristics common to Chinese party elites and their differences; developments under the People's Republic.

The author has displayed remarkable skill in locating a variety of sources, many not usually identified as having material on the Chinese political scene. These are supplemented by data and interpretations from a number of experts and their personal files, as well as by material from the Hoover Library. Valuable perspective and information was obtained during the author's trip to the Far East in late 1950, when interviews were held with, among others, M. N. Roy in Dehra Dun, India, and Chang Kuo-t'ao in Hong Kong.

Mr. North shows that in both elites there were at first a majority of young, Western trained and oriented revolutionaries, many of them sons of scholar-officials, businessmen, or landlords, pushed into revolutionary party politics by the frustrations of a disorganized colonial economy. As time went on, more often than not their personal lot worsened (an exception to the more frequently encountered situation in which elite members have experienced upward social mobility), while the movement they had started and the widening impact of Western thought penetrated to new strata of the population. Lower-middle-class and peasant elements began to make their way into both the Kuomintang and Communist Party hierarchies, largely in the role of men of violence as well as through the increasingly more important channel to leadership: the army. (In China the symbol specialists were relatively few; organizers were about half the elite.)

Here the similarities cease. The Kuomintang permitted itself to come increasingly under the urban-minded control of those seeking personal economic advantage in business activities, while the Communist Party managed to transform itself from an "intellectually, oriented organization into a rural mass-oriented one." It made use of the ideological weapons inherent

in it but also attracted many young, Western-educated malcontents who were keenly aware of the problems of the rural village, their home environment. These problems were shared by most Chinese. In view of the polarization of the two political parties and the disorganization of Chinese society, the struggle eventually became civil war, and in retrospect it is not surprising that the Communists won.

On the basis of the findings of this study, what does the future hold? The author suggests some of the pitfalls the Chinese Communist elite must avoid, including the possibility that rushing the agrarian revolution might result in a peasant upsurge which would make the Communist elite, in effect, a counterrevolutionary force. Furthermore there is a likelihood that Chinese-Russian relations may deteriorate. By their actions elsewhere, the Russians are certain to work for a tightening of Russian discipline over the Chinese Communist movement. Helped by such studies as this—perhaps the equivalent of much military potential if our policy makers in Washington only know how to use them—we in the West can hope to exploit the antagonisms between Russia and China.

FENTON KEYES

Skidmore College

The Progress of Underdeveloped Areas. Edited by BERT F. HOSELITZ. Chicago: The University of Chicago Press, 1952. x, 297 pp. \$4.75.

The twenty-seventh Harris Institute on International Relations recognizing the widespread interest in the problems of underdeveloped areas called together sixteen economists, anthropologists, political scientists, sociologists, and governmental administrators under the chairmanship of Professor Bert F. Hoselitz to consider the problems inherent in the various technical aid programs and the role of the Social Sciences in them. The result is a volume of crucial interest to the Social Sciences, and perhaps particularly to sociologists, who incidentally have a small role in it. For implicit in these discussions is the query of what can the Social Sciences do to aid the development and implementation of technical aid programs. A query, it would seem, of overwhelming pertinence to the sociologist.

The formal presentations on the problems of social change in nonindustrialized areas as they come under the impact of technological change, have been organized by the editor around three major interests: Part I, the historical approach to economic growth; Part II, the cultural aspects of economic growth; Part III, problems of economic policy. The papers subsumed under these headings although of varying merit are

as a whole of unquestionable importance. It is unfortunate that the reader of this volume does not also have before him the mimeographed transcript of the round-table discussions which followed each paper, for these are at times more illuminating than the invited papers presented here.

In Part III the discussion and analysis of the technical aid programs is presented (Konrad Bekker, "The Point IV Program of the United States"; Samuel P. Hayes, Jr., "Personality and Culture Problems of Point IV"); and the problems respecting the implementation of these programs is raised and directed towards the Social Sciences. One response is found in the caution signs the anthropologists raise in Part II. They point to several cultural resistants to social change which administrators should be aware of such as social values and attitudes with respect to individual industry, honesty, accumulation of wealth and distrust of government as barriers to social change. (Ralph Linton, "Cultural and Personality Factors Affecting Economic Growth"; Melville J. Herskovits, "The Problem of Adapting Societies to New Tasks"; Walter R. Goldschmidt, "The Interrelations Between Cultural Factors and the Acquisition of New Technical Skills"); the opposition of elites to social change, (Robert K. Lamb, "Political Elites and the Process of Economic Development"; Linton; Goldschmidt); social structures as exemplified in family systems as barriers to innovation (Linton; Goldschmidt); the implicit ethnocentrism of westerners in their approach to certain peoples as "backward" creates hostilities which constitute barriers (Herskovits). This type of precautionary advice is largely seen as negative by the administrator (Hayes) who is looking for positive and concrete guidance.

The demand for guidance found the social scientists setting out a series of generalizations as a basis of programmatic action. Programs of change, they stated, must take into account customs of the established order (Herskovits, Goldschmidt). Programs of technical aid should involve the problem of cultural continuity (Morris E. Opler, "The Problem of Selective Culture Change"). The effects of technological change are that of breaking down the ties of group solidarity and subverting the native systems of authority (Goldschmidt). Peasants suspect attempts at improvement (Linton).

These generalizations, hardly in need of lengthy specialized effort, provide little in the way of guidance to programs concerned not with primitive societies but with relatively industrialized and nonindustrialized countries. The administrators of such programs are concerned with the behavior of farmers, businessmen, consumers, legislators, investors, voters, etc.

(Hayes). The administrator is in need of knowing not just what are the cultural barriers to change but what ways can be found to eliminate or surmount these barriers. He is in need of knowing how local leaders can be identified and how local organizations can be utilized, and in learning what are the most influential groups in promoting or retarding change.

The lone paper by a sociologist (Marion J. Levy, "Some Sources of the Vulnerability of the Structures of Relatively Nonindustrialized Societies to Those of Highly Industrialized Societies") does try to provide a systematic series of propositions applicable to relatively nonindustrialized areas. However, this paper while brilliant in execution suffers from a level of abstraction which provides little in the way of immediate working tools for the administrator.

The failure of the social sciences to provide the necessary guidance for these programs should not be construed as entirely their fault. It is as much an expression of the failure of the administrator to define his problems and express his needs. The Social Sciences under the glare of this examination show several lacunae. It is true that the social scientist can provide analytic data on the Todas, the Semangs and the Ganda, and historical discussions of industrialization of Canada and Europe (Part I), but what of the intelligentsia of Burma, or the businessmen of Pakistan, the legislators in Japan, or the peasants of Iran. The concepts and methods of sociological research on communities, communications, classes, attitudes, etc., need trying out in the culture areas of the underdeveloped regions of the world.

The objectives of Point IV Programs are examined in a discerning and wise statement (Jacob Viner, "America's Aims and the Progress of Underdeveloped Countries"). The author recognizes that the paramount interest of the United States in these programs is one of security, although he does point out that these interests are not without their humanitarian aspects. The difficulty of much of the social science discussion is that it avoids recognizing this very point which would permit it to see itself engaged in a value laden undertaking. The confusion of what Weber describes as preference statements and fact statements runs as a constant theme through the proceedings to muddy the diverse objectives of social scientists and policy-makers. The proceedings of the Institute are limited by its failure to consider several vital problems germane to its central interest. The demographic problems of underdeveloped areas are given only fleeting reference, and problems of communications which continue to bewilder foreign aid programs are

only tangentially touched upon. Both of these are basic to an adequate analysis of change, and to the implementation of programs designed to foster such change. A comprehensive statement on needed research developments might well have been included in the proceedings.

This volume, however, has brought together a valuable introductory attack on the developing field of scholarly interest in nonindustrialized areas, and has attempted to lay the groundwork for a much needed interdisciplinary approach.

SIMON MARCSON

Brooklyn College

Land for Tomorrow: The Underdeveloped World. By DUDLEY L. STAMP. Bloomington: Indiana University Press, 1952. 230 pp. \$4.00.

"No problem confronting us today is more important than that of matching the world's use of its natural resources with the needs of its people." This book, while containing a chapter on the world's people, deals with the resources side of the equation. Its discussion from the beginning is tuned to the thesis of President Truman's Point IV Program, which is a program interested in alleviating the poverty of the people living in the so-called "underdeveloped" areas of the world.

The question immediately raised by the author is, "What, in a practical sense, is to be defined as an underdeveloped area? How shall development be accomplished?" Is it by maximum output per unit area or maximum output per man-hour? In the most prosperous farming areas of the world, the United States, Canada, Australia, New Zealand, and Argentina, for instance, the output per man-hour is very high. In many of the highly developed agricultural areas, Northern Europe for instance, the output per unit area is very high. In some of the so-called underdeveloped areas, parts of China and Japan, for instance, output per unit area is very high but output per man-hour is low. In some of the underdeveloped areas, output per man-hour and unit areas are both low. In some areas there is still poverty when something near the maximum use of natural resources is made with whatever technology is practically applicable to the farming set-up. These combinations of facts easily lead to the conclusion that poverty is primarily due to the fact that there is too much population for the resources available. Other conclusions are that either population pressure in these areas must be lessened or new vast areas must be developed for settlement.

The author estimates that if all the undeveloped lands of the world, like the "dense

forests of Brazil" and the "wide-open savannahs of Africa," were developed so as to be as productive as the average well-farmed lands of Northern Europe, the world could support four times its present population, or ten billion people. If the development of such lands were economically feasible, this would of course mean a great shift in the locales of much of the world's population. Because such has happened in the adjustment of man-land ratios in the past, there is a widespread belief that it offers the solution of the present and future. In the face of this fact, the author asserts that there are greater immediate prospects of increasing agricultural output in the middle latitudes than there is in securing immediate help in the world food situation from much more difficult tropical lands.

There is a vast difference between uninhabited and habitable lands, and most uninhabited lands are unoccupied or sparsely settled for good and sufficient reasons. Equatorial lands, while vast and sparsely populated, could be brought and kept under cultivation only at a tremendous cost. Their development is more than a task for bulldozers. It is the task of conserving and building the soils, the adequate technical knowledge for which is not now available. Furthermore, the incidence of debilitating disease is high in the tropics for both men and animals. Still further, the primary products of agriculture from such areas could not be made available to urban and export markets without the construction of vast and extensive networks of transportation. This is not to say that the problem of the development and use of these lands should not be attacked and solved. But the cost of pioneering in these areas is bound to require great expenditures of capital which will probably have to be supplied by states or other highly organized groups. As the author says, "The battles in these fields are not yet won, indeed we do not know how the battles should be won."

All of this looks like a blue picture, but the author is not pessimistic. He sees opportunities to ameliorate the conditions of living and to improve the techniques of agricultural production in the poverty areas of the world. He sees considerable opportunities to increase both production and population in the already "developed" areas of the world. He doesn't even fear that "the white races are being swamped by colored—brown, black, red—races." The rate of population increase in the last century has been greater in Europe than in Asia and greater in America than in India. He believes that wise and judicious land development is a more practical and fruitful approach than to attack the population issue directly.

"In a world short of food it is surely clear

that what matters is the actual amount of food produced," and therefore the higher the output per unit area the greater efficiency of the farmer. The need is for the optimum use of every acre of land. This is being accomplished only where holdings are small enough to demand the maximum efficiency in the use of science and in the arts of husbandry. It does not exclude the use of modern mechanization, which today is feasible and actually being practiced, in fields of ten or twelve acres. It does require that machinery must be "redesigned to suit the conditions of good land management and not farm lands altered to accommodate high mechanization." This is the best answer for the world as a whole, "the solution to the problem of preserving the new lands of middle latitudes, and in due course and suitably modified through research and experimentation, of lands yet to be tamed in the tropics."

Adequate food of course is not the only need of underdeveloped areas, nor land the only natural resource. Good diet demands cooked food and cooking demands fuel. To this must be added water supply and sanitation, housing, heating and lighting and ultimately refrigeration, for the preservation of food. This means the development of petroleum, mineral and water resources, and even the development of manufacturing. The author reviews the areas of the world where these resources are underdeveloped and reaches the conclusion that "It may be that certain underdeveloped areas will be able to pay for their own development. Mineral deposits are, however, in many cases remote from settlement and their development requires transportation, even exportation. Furthermore, they are non-renewable. Water resources, on the other hand, are renewable and have greater potentialities. Even they are not as widely dispersed or as easily developed as land."

Under two chapters, "Salvaging the Old World" and "Preserving the New World" the author presents validation of his conclusions. He cites Denmark, once described as the smallest and poorest country in Europe, now supporting four million of the best fed people in the world on ten and one-half million acres, five-sevenths only productive farm lands. He cites the results of land planning in England during and since the recent war, under the "concept of the optimum use of every acre in the national interest." He names the crucial issue in the poverty areas of the world as being "how to break the vicious circle" of the peasant farmer who is so poor that he can't buy efficient implements or fertilizer, can't buy fuel and therefore burns his cowdung, and who therefore has poor crops and animals and no surplus for sale. The first step in breaking this vicious circle is for each country to plan the use of its land. This is

easier than to develop new geographical frontiers, chiefly because it depends so much more on technical know-how than it does on giant undertakings and immense expenditures of capital. Furthermore, some of agriculture's most serious problems have resulted from running too rapidly away from the folk practices of peasant farmers. The application of science and even mechanization to folk farming is something in which both peasants and scientists can participate. This would be modernized peasantry, or to use the author's description, "balanced, mixed farming in small holdings." He is convinced that a balanced type of mixed farming, probably in small units points the chief direction for both planning and aid programs, and further says that "maximum output per acre on all tillable lands of the world would achieve maximum production for the support of maximum population and would come nearest to producing food for all."

CARL C. TAYLOR

Washington, D. C.

The Politics of Agriculture: Soil Conservation and the Struggle for Power in Rural America.

By CHARLES M. HARDIN. Glencoe: The Free Press, 1952. x, 282 pp. \$4.00.

This is not an easy book to review, although the topic is one that should be of considerable interest to the sociologist and particularly to the one who has a basic concern with rural life. Perhaps the reviewer has too much difficulty understanding the basic frame of reference used by the author, possibly he was too much influenced by the meagerness of the stated objectives, it may be that he is not astute enough to appreciate the author's method, or he might have expected a style of presentation that would be ill-fitted to the subject matter under consideration. In any case he found it a difficult book to read and a hard one to review.

One should reasonably expect to get an overall view of a book from a careful examination of its table of contents, especially one which gives detailed subheadings for the parts of the various chapters as is the case with the volume under consideration. Therefore, let us focus attention upon this. To begin with the book is divided into three parts, namely, "The Interests," subdivided into nine chapters, "The Process of Policy Formation," embracing four chapters; and "Recommendations and General Interpretations," including only two chapters.

A more detailed consideration of Part I indicates that Chapter I, "Politics and Agriculture," discusses the political question, the soil conservation phase, and underlying issues. This is followed by a chapter devoted to "The Colleges of Agriculture," in which the colleges and poli-

tics, conflict over policy origins, collective political action, the position of the colleges of agriculture, the Agricultural Extension Service, Extension's response to federal programs, and emergent issues are the topics discussed. Then attention is devoted to "Extension and the Farm Bureau," with the policy issue, legal ties and informal understandings, pros and cons, bills of divorcement, the divorce—anticipations and probable consequences, the difficulties of divorce, and what policy? are the subtitles. In Chapter IV, "The Soil Conservation Service—The Land Doctors," the author attempts to paint a panorama of the Soil Conservation Service followed by treatments of its achievements and the tasks ahead, engineers on the land, and the administrative line. "Soil Conservation Districts" is the title of the next chapter in which anticipations and what happened and problems and potentialities are the subdivisions. This is followed by "The Soil Conservation Service in Politics" and "The Union of Conservation and Parity." The first of these treats the compulsion to play politics, political strategy and techniques, ideology, and interpretations of the activities of the Soil Conservation Service; and the second discusses the Production and Marketing Administration, how it is organized, the PMA variety of conservation, and the contrasts, conflicts, and recriminations between the Agricultural Conservation Programs Branch of this agency and the Soil Conservation Service. "Farmer Administration" and "The AAA-PMA in Politics" are the adequately descriptive titles of the other two chapters in Part I.

In Part II Chapter X is devoted to "The Politics of Administration in the United States Department of Agriculture," subdivided into discussions of the strategic role of appropriations, policy as an "organization product," the USDA replies to the Farm Bureau, the failure of singlehanded reorganization by the USDA in 1948, and the Department and the controversies of 1948. This is followed by "Congress Goes to the Farmer," which discusses the hearings in various parts of the country by Congressional committees in 1947, and "The Hoover Commission and Its Sequel." "The 1951 Reorganization" completes this section.

The two concluding chapters, grouped together in Part III, carry the titles of "Farm Family Living" and "General Political Implications." The first focuses attention upon the policy goal and proposes, as a substitute for those implicitly or explicitly being followed, "the improvement of farm family living on a soil-conserving base." Then the difficulties of farm planning, institutional obstacles, and the importance of ideology are each considered in turn. The final chapter on implications brings in the constitutional

system, group politics, politics and administration, partisan politics and the drive for power.

A short appendix, a note on abbreviations and terms, acknowledgements, and the indexes are also included in the volume.

The foregoing should make it evident that at considerable length and with what seems to be unnecessary repetitiousness the author deals with the struggle for power on the part of the U. S. Department of Agriculture, the Soil Conservation Service, the Agricultural Extension Service, the Colleges of Agriculture, the Agricultural Adjustment Administration (after 1945 the Production and Marketing Administration), the American Farm Bureau Federation, the National Farmers Union, and various of their divisions and leaders. Undoubtedly this is an important field of study and it may be that more discerning readers will be able to get a clearer understanding of the nature and results of it all than it has been possible for the reviewer to obtain.

T. LYNN SMITH

University of Florida

Graphic Regional Sociology: A Study in American Social Organization. By CARLE C. ZIMMERMAN and RICHARD E. DU WORS. Cambridge, Massachusetts: The Phillips Book Store, 1952. x, 206 pp. \$3.50

The central propositions in this book ("inspired by the brilliant monographs of Durkheim") are that America (meaning the United States of America) comprises seven geo-social regions, each possessed of its own social system, its own regional collective personality and its own regional state of anomie. From this diversity of regional organization stems the genius of American achievement, and in it lies a model of World order;—"the causal forces which have made us come back again and again toward the ideal of a united world" (p. 10).

Certainly the intention to step up the discussion of regions from levels of geographic description to sociological analysis (Chapter IX) commands the attention of sociologists. Whether the introduction of such concepts as social space, social time, and social bifocality fulfill this intention, however, doesn't seem to be convincingly established. The exploration of regional features for evidence of mechanical or organic solidarity is interesting and probably relevant. But "regional personality" shifts around: now the region has it, now some person who lives in the region! Maybe we should make up our regional mind!

Perhaps the most important sociological contribution is the last chapter, written not by the co-authors, but by Albert Pierce, who refutes

rather successfully the most frequent sociological criticisms of regionalism. But he seems to falter in his last paragraph by accusing critics of misunderstanding or confusion rather than acknowledging that a lot of criticism is invited by the unclear expositors of regionalism.

From 6 to 12 pages of textual material and a half-dozen pages of maps and charts are presented for each of the seven regions: South Appalachian-Ozark, Northeastern Urban Industrial, Cornbelt, Wheatbelt, Arid West, and Pacific.

No doubt after some future revision this "monograph" will be somewhat less vulnerable to attack at certain points and levels. At the moment, however, it cannot be overlooked that errors in grammar and spelling with other violations of the conventions of exposition are a reflection of carelessness that may be suspected to prevail also in thinking and uses of data. At any rate it would be better if they did not appear in books that students use (pronoun without antecedent, p. 126; plural subject and singular verb, p. 92; letters omitted, p. 125; etc., etc.).

The irrelevant as well as the relevant items are reproduced from the coverage of an experiment station bulletin (p. 105) . . . The authors' interpretations are inserted below borrowed maps but above the credit lines to the maps.

Several pictographs (e.g., pp. 6, 104, 106) lead one to suspect that the book may have been designed in part for pupils in elementary schools. The general structure of the book (like that of a magpie's nest) is more mechanical than organic.

HOWARD W. BEERS

University of Kentucky

A Methodological Study of Migration and Labor Mobility in Michigan and Ohio in 1947. By DONALD J. BOGUE. Oxford, Ohio: Scripps Foundation for Research in Population Problems, 1952. vi, 100 pp. \$1.00.

This monograph describes the results of a special tabulation made at the Bureau of Old-Age and Survivors Insurance. The insurance records on which the tabulation was based are described in a chapter contributed by Saul D. Hearn and Paul Eldridge of the Bureau. Briefly, information from the worker's application for an account number, the employer's application for an identification number, and the employer's quarterly tax return is brought together and punched on the "annual employee card." From this card, each change of industry, employer, or location of employment during the year and within covered employment can be obtained. These cards represent a permanent one per cent

sample of all the employees who have had work in covered employment since 1937.

The present study is confined to those employees in the sample with some covered wages earned in Michigan or Ohio in 1947. This segment of the sample comprised 54,737 cases.

Mobility is examined in terms of (a) change of county of employment (migration), (b) change of industry, and (c) change of employer. In terms of comparisons of all four quarters, about one-third of all workers in Michigan and Ohio were mobile in one or more of these senses. About one-sixth were migrants. About 85 per cent of the workers who were mobile in any sense changed their industry of employment, "industry" being defined in terms of a 78-category system. (It would have been helpful to the reader had this system been listed in an appendix.) Practically all of the mobile workers changed employers.

In most of the tables, mobility is determined by comparisons of the first and last quarters of employment in the year. Furthermore, most of the analysis is carried out in terms of a set of three mutually exclusive categories: *migrants*, nonmigrants who were *industry-mobile*, and workers who were *employer-mobile* but neither *industry-mobile* nor migrants. Here total mobility becomes the sum of these components. Using these definitions, we find that migrants constituted 13 per cent of all workers; *industry-mobile* workers, 12 per cent; and *employer-mobile* workers, 8 per cent.

In addition to data for the State totals, Bogue also presents consolidated distributions for counties grouped into 18 metropolitan and 16 non-metropolitan areas. These areas are an earlier version of his State Economic Areas, which have been adopted by the Bureau of Agricultural Economics and the Bureau of the Census. The fact that rather different patterns of mobility were frequently observed in specific areas (Chapter XI) suggests that generalizations made for Michigan and Ohio may not apply very well to other parts of the United States.

Five chapters deal with mobility in relationship to various characteristics of the worker. These are sex and race (Negro or non-Negro), age, type of industry, pattern of quarters of employment, and annual earnings. Each chapter ends with a section dealing with the net effect of mobility upon the composition of the covered labor force with respect to the given characteristic. Sometimes the bad practice is followed of showing the percentage distribution of net migrants when there are both positive and negative frequencies.

Workers who had been in covered employment in noncontiguous quarters were particu-

larly mobile. There are some slippery conceptual problems here including the evaluation of the effect of length of exposure to risk. Annual earnings are defined as wages earned in covered employment. Since, prior to 1951, wages in excess of \$3,000 a year did not need to be reported, there is a rather larger proportion of cases in the open-end interval, "3,000 and over." Roughly speaking, workers whose annual earnings fell in the second lowest quartile for their group tended to be most mobile.

The chapter titled "A Multiple-Variable Analysis of Migration and Industry-Mobility" analyzes mobility rates by each of the above characteristics standardized for one or more of the other characteristics. The previous patterns cannot be entirely "explained" by these standardizations; but standardization for annual earnings strikingly reduces the differences within rate patterns of other characteristics, particularly in the case of age.

The final chapter contains 14 "labor mobility hypotheses." Most of these are generalizations based on relationships observed in the study. Bogue discusses "intrafirm mobility" and "occupational mobility" as additional types of labor mobility. B.O.A.S.I. records provide no information on occupational mobility; and, in fact, none of the kinds of mobility studied in this monograph would be classed as "vertical" or "social" mobility by sociologists.

Perhaps one of the reasons why this vast body of material has been neglected by demographers is that the universe covered does not correspond to any of their usual concepts. In 1947 about two-thirds of all workers were in covered employment in at least one quarter, but important segments of the labor force were not represented. As the coverage of Social Security is expanded, this limitation will become less important and a decreasing fraction of all labor mobility will be unreported. This study does not use some of the unique potentialities of the B.O.A.S.I. data. For example, mobility is not related to annual earnings in the year *before* the move. There is very little on streams of mobility, such as those between two specific industries.

Nevertheless, the Bureau of Old-Age and Survivors Insurance records are valuable as the only extensive source of annual data on mobility for small areas. Moreover, they could be the basis of a major bridge between the study of the labor force and that of migration. It is to be hoped that the research interests of scholars will combine with the resources of the Bureau of Old-Age and Survivors Insurance to produce continuing tabulations of this material.

HENRY S. SHRYOCK, JR.

Bureau of the Census

The American City. By STUART A. QUEEN and DAVID B. CARPENTER. New York: McGraw-Hill Book Company, 1953. viii, 383 pp. \$5.50.

Persons interested in urban sociology are indeed fortunate in that there is now available a number of general works in the field suitable both as texts for classroom purposes and as summaries of the current status of research. Within the last two years there have appeared five books of this type. Two are books of readings, two are entirely new single-author titles in the field, and the fifth, which is the book under review, may be regarded either as a new work or as a complete revision of a previous work, as one pleases. In one sense it is a revision—it replaces *The City*, co-authored by Queen and Thomas in 1939. Although Carpenter has replaced the late Lewis F. Thomas in the collaboration, his predecessor is given appropriate recognition in the preface with the statement that "his influence continues to be reflected throughout the present work." As the authors point out, "The approaches of the urban sociologist and ecologist continue to dominate the treatment." This is reflected in the fact that the general organization of the book remains pretty much the same. Many chapter headings are virtually unchanged. The work continues to bear the stamp of the "Chicago School" (Park, Burgess, McKenzie, *et al*).

Nevertheless, there are a number of significant changes which probably justify regarding it as a "new" contribution to the field of urban sociology. Its newness revolves mainly around the reorganization of the basic frame of reference in terms of (1) a sharp distinction between urbanism (number and density of population) and urbanization (the urban way of life); (2) the construction of a hypothetical rural-urban continuum correlating urbanism and urbanization—with small numbers, homogeneity, and "primary" contacts at one logical extreme and large numbers, heterogeneity, and "secondary" contacts at the other; and (3) the employment of a rigorous empirical methodology for testing the above hypotheses, in which an Index of Urbanism is set up utilizing county data from the continental United States based on the 1940 census.

Queen and Carpenter draw heavily upon the research of Redfield (*The Folk Culture of Yucatan*) and Loomis and Beegle (*Rural Social Systems*) for their conceptions of the rural-urban continuum, but go beyond these in constructing their own index of urbanism which is both ecological and quantitative. In this way they approach interaction, relationships, and norms indirectly. They believe this to be more practicable, although "this procedure leaves much to be desired."

Their Index is the arithmetic mean of 10 sepa-

rate measures of urbanism—population percentages resident in places ranging from 500 persons to those with 500,000 or more. The validity of their Index is tested by correlations with "six other commonly used measures of urbanism" and with quite satisfactory results (the specific correlations range from .75 to .86).

The hypothesis of the rural-urban continuum is further tested by localized data from the Pacific Northwest (data from Elma, Washington obtained by the junior author in an earlier research study). A composite scale of rurality is constructed and validated by means of the Guttman scalogram technique. Linear correlations between rurality-scale and seven other scale scores are sufficiently significant to lead to the conclusion that: (1) "There is a continuous gradation in the United States from rural to urban rather than a simple rural-urban dichotomy and (2) as human communities are arrayed along this rural-urban continuum, consistent variations occur in patterns of behavior." Although they are not clear on the point, the authors seem to regard urbanism as the independent variable.

The above general hypotheses serve as the central theme of the book. Less attention is given to urban institutions as such and more emphasis to the problems of urban adjustment: Making a Living; Making a Home; Getting Help in Time of Trouble; etc. Chapter 15, Achieving Status, represents a good summary of empirical research on social status and class structure and is a valuable addition to the work.

In addition to its explicit and systematic theoretical structure grounded in careful empirical research, the book has other merits, particularly as a text for classroom use. The style is clear, lucid, and uncomplicated. The introductory chapter, Importance of Studying the City, should intrigue and motivate students very effectively with its vignettes of Detroit, Chicago, and Boston.

The work does have its weaknesses and limitations, however. The principal defect is to be found in its concluding section—there is a slight tendency for it to "peter out." Urban change is handled in a somewhat superficial manner, as though to hold down length (there are only 383 pages compared to 500 in Queen and Thomas) and to avoid questions that are admittedly speculative and pervaded by value judgments. The final chapter, Social Planning of Cities, is brief indeed, and shows little relation to the original theoretical framework. Nevertheless, this is by far preferable to dogmatic preaching.

Although objectivity is maintained throughout, it is evident that the authors, on balance, find cities to be desirable elements in our mode of life. The absence of the doctrinaire is well evidenced by the fact that the authors avoid pitting

the concentric-zone theory of spatial patterning against the sector theory, for instance. They are eclectic about the matter: "all four devices—topography, sectors, nuclei, concentric-zones—are useful schemes for describing, analyzing, and comparing the spatial patterns of large cities." They restrict their generalizations to the American City, but see the value of cross-cultural comparisons.

HAROLD W. SAUNDERS

State University of Iowa

Readings in Marriage and the Family. Edited by JUDSON T. LANDIS and MARY G. LANDIS. New York: Prentice-Hall, Inc., 1952. xv, 460 pp. \$4.25.

This book of readings will be welcomed by many teachers of courses on Marriage or The Family who have been faced with inadequate library facilities for their students, and who wish to give uniform supplementary reading assignments which can be tied in with the rest of the course. Seventy-five selections have been included in this volume, mostly from periodical articles, the majority based on research. The emphasis in selection can be most easily indicated by listing the 16 chapter headings and the number of selections in each. There are chapters on: The Contemporary American Family (5), Perspective on Marrying and the Family Cycle (3), Dating and Courtship (6), How Mates Are Sorted (5), Predicting Marital Adjustment (3), Weddings (3), Husband-Wife Interaction (9), Mixed Marriages (2), Family Reproductive Behavior (8), Family Interaction—Parents and Children (7), Family Problems and Crises (7), Divorce (6), What Role for Women (4), Standards of Sexual Behavior (3), Aged Family Members (2), and Family Life—Education, Counseling, and Research (2). Except for three brief items dealing with Hindu marriage and with mourning customs in Bengal, all selections treat the modern American family. As can be seen from the chapter headings, emphasis is primarily on the courtship, mate selection, marriage adjustment cycle, with comparatively little material which would represent the institutional or the structure-function approach to the family.

In a field where there is so much material to choose from, it would be easy to criticize the exclusion of some items and the inclusion of others. For the most part, the selections in each chapter have been well made. There is a sufficiently wide range so that the instructor can find plenty of things which he would want his students to read. It is more likely that users of this book will criticize it from the standpoint of the amount of cutting of many of the selections. The authors and publisher of a book of readings are faced with an unpleasant dilemma.

If they wish to include a large number of selections, they must either have a very large and expensive book that reprints complete articles, or a smaller book with considerable editing of the selections. The latter course was chosen for this book. Many of the articles have been reprinted in fairly complete form, but others have been drastically cut, omitting much of the discussion of methods, data, and interpretations, leaving only brief introductions and conclusions. Not only does this make for very highly condensed reading, but the critical student will often wonder how it was that the author arrived at his conclusions. This treatment inevitably limits the achievement of one of the goals stated in the Preface, that "it is to the advantage of the student to seek objectivity and a better perspective by reading the reports of research as offered by the researchers themselves." Working within this limitation, however, the authors have done a skillful job of editing to preserve continuity of discussion and pick out the most relevant conclusions.

Particularly since this book has no competitor at the present writing, it can be safely predicted that it will find wide adoption as a supplementary text in undergraduate courses in Marriage and the Family.

CHARLES E. BOWERMAN

University of Washington

Your Marriage and the Law. By HARRIET F. PILPEL and THEODORA ZAVIN. New York: Rinehart and Co., Inc., 1952. xv, 358 pp. \$3.00.

A Psychoanalytic Lawyer Looks at Marriage and Divorce. By JOHN H. MARIANO. New York: Council on Marriage Relations, Inc., 1952. 276 pp. \$3.00.

The book by Mrs. Pilpel and Mrs. Zavín, both of them lawyers, is an excellent statement for non-legal students of marriage of some of the most pertinent legal facts-of-life. It is divided into four parts, namely, Husbands and Wives, The Children, The Sex Side, and Termination of Marriage. It is modern enough to include a chapter on the legal aspects of artificial insemination. It is always fascinating to watch the legal mind at work, and this clear, straightforward presentation, illustrated with specific cases, is like a loge at the theater. There is no doubt that the law is grappling with a maelstrom of human emotion, of churning passion. But its tools seem so inadequate, so inappropriate, so beside the point in the light of what we now know about human personality. No wonder judges and lawyers alike are overcome with frustration. They are trying to negotiate a stormy sea in a raft with reeds for poles. Like

all conscientious lawyers, Mrs. Pilpel and Mrs. Zavin advocate marriage counseling before marriages go on the rocks rather than legal redress afterwards. There is no index; there is a brief summary of grounds of divorce in the 48 states and the District of Columbia.

Mariano straddles both the marital counseling and the legal professions. He is therefore acutely aware of the limitations of both. He feels that the breach is widening between the sociological techniques available for family therapy and existing legal remedies. He believes that the social-work approach and the equity-law technique are both deteriorating, the first because of high case loads and inadequate professional requirements for practitioners and the second because of the lag between legal rigidities and social habits. He states that the law of divorce was molded primarily for handling breakdowns in families in the underprivileged classes but that we now need to shape the law to deal with family breakdowns resulting from neurotic personalities in other classes as well. Psychological rather than budgetary considerations should receive more consideration. He advocates a "psychotherapeutic jurisprudence" which "is based upon codification of mental and emotional causes and factors responsible for marital weaknesses, and which while fixing responsibility does not level blame" (p. 260). He would like to see the law capable of dealing with personality needs rather than only with legally defined deviations from legal norms.

Mariano's thinking is, of course, in line with that of most students of family life; but it is probably still anathema to most lawyers at the present time. The conservative, tradition-oriented lawyer fears the amount of discretion required in administering a flexible law. The injustices which result from applying a rigid legal rule to infinitely varying human beings seem to him less serious than the injustices which would result from applying a flexible rule.

Mariano belongs to the Bergler or "divorce-won't-help" school of thought. Although no doubt valid for a certain segment of the population, this point of view does not hold any more than other dogmatic generalizations about human behavior. It is doubtless true, as both Bergler and Mariano point out, that there are some people who are, for neurotic reasons, incapable of successful adjustment to marital relationships. Divorce will not necessarily help them. It may even damage them. They are probably the cases which tend to swell the divorce rate among the remarried population. But to generalize from this group to all married people is quite fallacious. There is evidence that divorce *will* help in an as yet indeterminate proportion of cases. If the divorced person remarries, the chances are not too unfavorable for

a successful marriage. We do not know the exact proportion of divorced persons who remarry; the estimated percentage varies from one-third to one-half. In a study of 2009 cases of remarriage, whose success was rated by informants, 47.9 per cent of the divorced men (788) and 50.6 per cent of the divorced women (828) were judged to be above average or extremely successful. In their cases, divorce *did* help.

Mariano makes a valuable contribution, however, in contrasting the human, psychological, and sociological factors in marital failure with the formal, legal ones. He urges lawyers who deal with divorce cases to apply the newer counseling techniques of psychotherapy where possible, although he makes clear that they should not attempt to play the role of psychiatrist unless properly accredited to do so.

JESSIE BERNARD

The Pennsylvania State College

Human Relations: Vol. I, Concepts in Concrete Social Science; Vol. II, Cases in Concrete Social Science. HUGH CABOT and JOSEPH A. KAHL. Cambridge: Harvard University Press, 1953. xxxi, 333 pp. and viii, 273 pp. \$4.75 and \$4.25, respectively.

Eighteen members of the Harvard University staff, most of them from the Harvard Business School, have brought together ideas and evidence from a number of different social disciplines in these 18 readings by 16 different authors plus 33 specific "cases" in these two volumes to provide the basis for a discussion course in "the face-to-face interactions which occur between individual men and women in their immediate social environment." Volume I presents the concepts from different disciplines which are to be used in interpreting the cases in Volume II. An Introduction of 14 pages defines human relations in the terms quoted above and states the theory on which the undergraduate course in Harvard College on human relations, Social Sciences 112, has been operating since 1945.

To begin with, the authors note that unlike the student's relation to the phenomena under study in the natural sciences, the student of human relations and to some extent of all social sciences is already familiar with the data to be considered and has reached his own conclusions about them. Hence, "a proper spirit of humility regarding the notions of human relations is, therefore, much more difficult to achieve than in other fields of study . . . we must not only attempt to *evaluate* data in ways that the specialists have found to be productive but we must also try to *reevaluate* very similar data that we have found for ourselves to be fundamentally

important in our own lives" (p. xxii). To bring about this reevaluation, the authors and their associates have found case discussion a useful method and are convinced that students gain the most from it when they are forced to take up specific cases without printed study questions to guide them.

The scheme in Volume I, *Concepts in Concrete Social Science*, is threefold: first, for each of the 13 chapters there is a brief orienting discussion by the authors, and a list of additional readings, then in the first 11 chapters come selections from 16 authors on the basic concepts involved in The Clinical Approach, Difficulties in Clinical Observation, Cultural Values and Social Roles, The Responsibility of Decision, Our Developing Social Interactions, Thought Processes, The Individual's Response to Stress, Group Membership, Group Processes, Value Conflicts (illustrated mainly by selections on race relations and anti-Semitism), and Reevaluating Sentiments. For the last two chapters, Executive Leadership and Social Control and Equilibrium, the student is referred to Barnard's *The Functions of the Executive* and to Homan's *The Human Group* and to Whyte's *Pattern for Industrial Peace*. Authors in the list of selections in the first 11 chapters include Durkheim, Freud, Cooley, Mayo, Horney, Lewin, Florence R. Kluckhohn, Warner, Gardner, and others.

Volume II consists wholly of the 33 cases, presented in from 3 to 34 pages each without comment. Cases range from a college dean's puzzlement whether to recommend for graduate study a student who may not be "quite the type" to the incidents in a patient's day in a hospital, the problems of running a community fund drive, and a number of business situations. For the most part, the situations presented never depart very far from middle class decorum and raise few very basic questions.

This is frankly not an introduction to sociology but rather an introduction to a very practically-oriented study of face-to-face relationships by means of class discussions of specific situations which happen to have been chosen for attention. Now the case method of instruction has a long and honorable history in law and medicine, and for a number of years has been used in the Harvard Business School. But in the older disciplines, at least, it is based on a systematic body of theory. In the present instance the theory is anything but systematic and there is nothing to show that other concepts such as Sumner's folkways, Cooley's primary group, and so on, might not have been used with equal effectiveness.

The dust jacket reveals the real thrust of these two books: "the comprehensive nature of this work lays a broad foundation for study in

such diverse nonacademic groups as those formed by business executives, labor leaders, personnel directors, government officials, civic minded laymen, and the clergy." In other words, these books are tools for helping students and practical people gain some insight into the mechanics of interactive relationships face-to-face. They do not introduce the student to the scientific methodology of such study nor do they attempt to orient the student to the place of face-to-face relationships in the broad panorama of human association. As a basis for discussion, however, they provide much useful material.

L. J. CARR

University of Michigan

The Case Method of Teaching Human Relations and Administration: An Interim Statement. Edited by KENNETH R. ANDREWS. Cambridge: Harvard University Press, 1953. xvi, 271 pp. \$4.50.

This book consists of a series of essays describing the experiences and philosophy of members of the Harvard Graduate School of Business Administration in using the case method of teaching human relations in administration. Since it is labeled an interim report, any critique must be regarded as tentative and possibly unjust. However, the content and orientation of the essays show clearly that the teaching approach is an intellectual grandchild of Elton Mayo and is subject to the values and limitations of his school which have been commented upon by many writers. One senses throughout the book a missionary desire to propound the "new administration" as set forth by Mayo.

It should be emphasized that the subject matter which is the primary focus of this book is not sociology but rather the psychology of interpersonal relations. While formal organization is recognized as one factor in administrative behavior, the major concern is the analysis of how individuals do act and "should" act toward other individuals in the accomplishment of specific tasks. Consequently, student discussion tends to become an exercise in amateur psychoanalysis in an effort to sort out personal motives, attitudes, and qualifications. This individualistic basis for problem solving is actually encouraged by staff emphasis upon the uniqueness of each case situation and the deliberate discouragement of the use of principles or generalizations for analytical purposes.

This is not to deny the value to the student of being forced to a consideration of concrete problems and of the steps which an administrator must take. Few will deny the position that case materials give a much closer approximation to "reality situations" than lectures or

discussions based upon theory or carefully abstracted sets of data. The selections in *The Administrator* (the cases upon which the present book is based), illustrate a wide range of institutional settings and interpersonal problems. An area which could be expanded is problems arising from collective bargaining. These certainly constitute a major concern of the business administrator today.

The essays dealing with student confusion and resistance in the initial exposure to the case method afford valuable insights into the ways in which these aspects of teaching can be treated. A more detailed presentation of the content and extensiveness of student counseling would be desirable. It is possible that the value of any case to the student would be enhanced by an opportunity to make an administrative decision upon the basis of case content and discussion and then to compare his decision and the assumed results with whatever decision actually took place and its effects. This would intensify the student's feeling of involvement in the situation and would give him an opportunity to check his decision with observed administrative behavior.

The authors state explicitly that they do not have a general theory of case teaching or of administrative practice and that their purpose is to encourage free thought and individual solutions of administrative problems. This goal does not seem quite consistent with the emphasis upon HGSA doctrine and the criteria described for passing and failing student performance. The reviewer is also alarmed at the anti-scientific and anti-intellectual connotations of the stated reliance upon an "inner sense" or intuitive appraisal of operating situations which is divorced from logical or systematic analysis.

DONALD E. WRAY

University of Illinois

Factory Folkways: A Study of Institutional Structure and Change. By JOHN S. ELLSWORTH, JR. New Haven: Yale University Press, 1952. v, 284 pp. \$4.00.

In *Factory Folkways* the author undertakes a dual task. First, he describes the history of an institution, the "New Freedom Products Company," through the more than hundred years of its life. Then he goes on to develop a theoretical scheme to apply to the analysis of such an institution.

The history of institutional development Ellsworth covers rather briefly (eighty-six pages). He devotes more than twice this much to the exposition of his theoretical scheme. Unfortunately, it seems to the reviewer that the rather brief history is exceedingly valuable

while the extended theoretical discussion adds little if anything of value.

Ellsworth's study is based upon several years of experience in the plant, first as a management trainee, and finally as personnel manager. Upon returning to graduate work in 1942, Ellsworth kept in touch with the plant and went back for further interviews and observations. The book is thus based upon a rather intimate knowledge of an organization and this knowledge is well used in telling the story of institutional growth and change.

We see here the growth of a small factory as a family business, with very close personal ties between owners and workers. With the growth of the company and with the increasing complexity of the managerial job and the introduction of management people who have never had close contacts with the employees, we see a gradual breaking down of the early close personal relationships and a separation of the institution into two distinct parts. Finally, we see the rise of a union organization in reaction to some of these changes.

This sort of institutional development can, I am sure, be duplicated many times over, but this is the first time, to my knowledge, that it has been carefully documented. That is the particular value of Ellsworth's book.

On the theoretical side, Ellsworth bases his work upon Malinowski's concept of institution, and also utilizes some of the ideas of E. Wight Bakke and Sumner and Keller.

In his concluding chapter Ellsworth says, regarding his use of Malinowski's approach, "The theory consists of a concept, or a set of related concepts, which defines an institution and outlines in broad terms the various sorts of factors which must be taken into account in interpreting or explaining behavior. . . . In essence, then, Malinowski's institutional conquest (*sic*, concept?) provides a chart or check list for research."

This clearly states the limitations of what Ellsworth has sought to do. The concepts he utilizes hardly present a theoretical scheme for the analysis of an institution. At best, they present an approach to research. They provide a number of boxes or categories into which research data can be divided and sorted out. However, the ability of the researcher to sort out all his data into these boxes hardly proves the value of his scheme. When we have filled our boxes, we have taken the institution apart, but we have not shown the dynamics of an institution in action.

Ellsworth seeks to pull something together in the concluding section entitled, "Dynamic Aspects," but aside from calling his historical data by different names this seems to add nothing.

ing to the story of institutional growth and development as it is first presented.

WILLIAM FOOTE WHYTE

Cornell University

Three Studies in Management. By JEROME F. SCOTT and R. P. LYNTON. London: Routledge and Kegan Paul, 1952. x, 220 pp. 18 s.

This modestly titled book achieves a degree of systematic treatment and generalization quite at variance with the usual "case studies" of organization.

The three studies to which the title refers are themselves selected and presented in a kind of natural history sequence: (1) a coal mine as an old and traditionally oriented organization undergoing recent changes; (2) a small factory recently introduced into a mining community; and (3) a large factory with a long record of frequent and rapid changes in technique and organization.

In each of these studies the data are presented in terms of four cumulative categories: organization, communication, control, and adaptation. The three studies are then compared by use of these same categories. The comparison yields a number of generalizations, such as the constancy of certain kinds of cleavages and conflicts of organizational principles, the effects of size, and the necessity of top-level changes if bottom-level changes are to be effective.

The book is further commendable for the introductory comments about the changing environment of industrial enterprise. Indeed, perhaps most commendable is the application of the research approaches characteristic of industrial sociology to *managerial* organization. Much of the literature of the field seems to assume that "management" is completely homogeneous and unproblematical.

Precisely because this book is very good in an unpretentious way, I should like to state some difficulties and objections. The authors seem to me to be too thoroughly tied to the Mayo tradition. For example, it is surely time to recognize that "informal" organization is a residual category as it is usually treated. The assumption that "informal" organization is spontaneous, flexible, and based on wider ego-involvement than the "formal" may not be true, given personnel turnover. Indeed, this dichotomy leaves out of account the very important phenomenon of precedent in an organization. This may be saying simply that the conceptual apparatus is "too static" but one should not be content with uttering the epithet. The authors have been very much concerned with change, but I believe they have not sufficiently followed the implications of change for the conceptualization of organization.

Some further difficulties derive from the methodological appendix. Here the authors follow Mayo's radical empiricism by alleging that science starts with observation followed by speculative hypothesis. Some bits of science do, but the statement is too categorical. I venture to deny that this portion of the authors' methodological note bears any resemblance to their actual procedures. In this same note Scott and Lynton discuss the methodological problem of the observer as part of the field of observation. But this is surely true only for research relating to low-level generalizations, and even here there are checks. Interviewing, incidentally, is not the sole source of sociological data or the sole technique of current observation.

To make the last point clear I should like to quote a passage from the authors' introduction. I quote with approval and with the comment that no observer's influence is likely to affect the validity of the observation:

An individual tends increasingly to be worker, member of a family, citizen, scholar, and hobby enthusiast each at a different time of the day, in a different institution, and in association with a different set of people. This at once multiplies both the need and the difficulties of adjustment. It also threatens that the different situations to which people have to adjust may make conflicting demands; the different facets of life may no longer be compatible.

WILBERT E. MOORE

Princeton University

Soviet Economic Institutions: The Social Structure of Production Units. By ALEXANDER VUCINICH (Hoover Institute Studies, Series E: Institutions). Stanford, Calif.: Stanford University Press, 1952. x, 150 pp. \$1.75.

It is, perhaps, inevitable that, to people on this side of the "iron curtain"—especially to Americans, the doings of Soviet Russia possess a pervading aura of unreality. There are things within Russia which we can understand—or almost understand. But when we try to take hold of them, they have a habit of dissipating into a scatter of Russian words, cryptic abbreviations, and chameleonlike concepts which shift their meaning from occasion to occasion until we wonder whether life can really be lived as it is said to be lived by the peasants, the workers, and the intelligentsia of this strange half-world. At least we can say there are some things in Russia which are comprehensible to us, but even this statement is a guess. The iron curtain is more than a political barrier. It is also a barrier to knowledge.

In this study, Dr. Vucinich attempts to penetrate this barrier, to pierce it, so to speak, with one clear window. This window he constructs from six types of Soviet materials: (1) collec-

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tions of government and Party decisions, (2) juridical monographs, treatises, and journals, (3) the official organs of "The Party," of trade unions, and of the Young Communist League, (4) the official journal of the Procurator, (5) professional economic studies and journals, and (6) semi-popular journals which carry "belles-lettres" and studies of selected economic and social problems. Whether the panes which these sources furnish for Dr. Vucinich's window are as clear as he would like remains a question. The confusing effects of strange terminology and alien conceptualizations still remain. But, nevertheless, the picture is much clearer and more plausible than most of those we receive. It is not, of course, complete, even within its own scope, for, as Dr. Vucinich remarks of informal organizations within the factory, there is a good deal which cannot be known without intensive field work—a kind of research which, for obvious reasons, cannot be done.

Within its limitations, then, the book is valuable material for industrial sociologists in particular and for all students of organization. The specific subjects covered are:

(1) The factory, including its administrative and control mechanisms, its division into various social groups and the possibilities for social mobility, and the evidence that informal organizations exist. These last, strictly speaking, are those which the official Soviet writers exorcise as "deviant". They comprise organizations which resemble informal groups described by American industrial sociologists, such as those created to restrict production, and also organized thieving groups of workers, supervisors, and officials.

(2) The *Kolkhoz*, which includes three types: the completely socialized agricultural commune; the agricultural *artel*, ostensibly based on principles of co-operative democracy; and the agricultural association for joint cultivation of land.

(3) The *Sovkhoz*, that is, the agricultural enterprise owned and operated by the state. These are supposed to be urban settlements dispersed throughout the countryside and to serve as technological and social models toward which the "*Kolkhozniki*" may strive.

(4) The Machine and Tractor Station, which is the principal state organization participating in agriculture and is intended to be not only a farm-machinery organization but also the means for transforming rural society to socialism.

(5) Industrial and Artisan Co-operatives, which, although important in Russian production, are inconsistent with Communist ideology and are regarded as convenient means for bringing traditionally independent craftsmen into the orbit of socialism.

(6) Taken together, Dr. Vucinich's descriptions are highly revealing and raise some ex-

tremely interesting questions. Among these are the validity of line, staff, and functional organization for economic enterprises in general; the basic problem of the relationships between voluntarism and coercion in promoting organized activity; and the ways in which class stratification occurs—ways which even in Russia seem to differ from the orthodox Marxian prescription.

JOHN S. ELLSWORTH, JR.

Yale University

De Ongeschoolde Arbeider: Een sociologische analyse. By J. HAVEMAN. Aasen: van Gorcum and Company, 1952. 224 pp. Fl. 8.50.

This study, which originated as a thesis at the University of Groningen in the Netherlands, presents a sociological analysis of unskilled workers in several towns and rural districts of North-eastern Holland. As a case-study, resulting from the very active school of Dr. P. I. Bouwman, it is an achievement of merit because very few of this type of studies have been undertaken in Holland and they are of considerable value in a society which is certainly not "sociology-conscious."

This reviewer has some reservations, however, in regard to the theoretical framework as well as the presentation of the data. The author defines his hypothesis as follows: "The unskilled workers form the nucleus of a social class in which many give evidence, by behaviour which is not in accordance with the bourgeois group-norms, of the fact that they either are not aware of, or do not acknowledge, the traditional social norms in the Netherlands" (English summary, p. 218).

This hypothesis contains quite a few presuppositions about which there would be no reason to argue if they belonged to generally accepted sociological theory or if they were presented within a theory that would carry a reasonable amount of conviction. The first assumption is that a national group, i.e., the Netherlands, has a system of "traditional social norms" from which one group, viz., the unskilled workers, deviate. As a result, the author stresses the "bourgeois" pattern of the other groups which stands in contrast to that of the unskilled workers.

This, certainly, is an over-simplification. Logically, a national pattern belongs to the entire nation-group which may actualize it to a varying extent in its various classes, but it does not belong only to some classes and not to others. In that case, economic class would be the determining factor, and there would be no valid reason to bring the national pattern into focus.

Besides, it is assumed further that the nationality pattern is a middle class one from which, obviously enough, the unskilled workers

deviate, though the writer implies that the solution would be to attempt to give middle-class attributes to the group in question. This does not conform to the structural aspects of a social group since relatively large independence of the various classes would be a prerequisite for giving them all middle-class aspects, which is only possible if the term implies a set of values rather than a class attitude. It is evident from the study, however, that the writer is aware of the relatively high complexity of the Dutch social structure so that the dichotomy he uses does not do justice to the problem which he attempts to analyze.

It is made clear that the attitude of the unskilled worker is a function of the social system as a whole although the entire study views the problem far more in relation to an economic class system than one which has predominantly national characteristics.

In addition, the organization of the data has a number of weaknesses. The group which was studied is not set off clearly, either statistically or in terms of the entire social system. There is also relatively little attention paid to the structure of the group itself, and to the transitional layers leading up to the skilled workers.

The point is stressed strongly that primary groups are of great importance to the unskilled worker, but it does not become clear whether this is specific to the studied case or whether it should be regarded as a generally valid conclusion. In the latter case, one would wish an explanation since underprivileged groups have shown themselves quite capable of organization so that the correlation between low class status and great stress on primary groups may not be a generally convincing one.

It must be said again that the study deals with an unmapped territory, and as such it is of considerable significance in spite of a certain lack of clarity in its theoretical formulation.

BART LANDHEER

Peace Palace, The Hague

Strikes: A Study in Industrial Conflict. By K. G. J. C. KNOWLES. New York: Philosophical Library, 1952. xiv, 330 pp. \$8.75.

The author has written a thorough and scholarly book on the strike, with particular reference to British experience. The American reader will also find frequent references to our own strike experience. The volume is divided into two parts. The first half of the book is a descriptive analysis of the strike, treating such items as the strategy and tactical forms of strikes, the effect of trade unions and employers' organizations on industrial militancy, the changing role of the state in indus-

try, and the operation of restrictions imposed by government, management, and unions.

Part B is concerned with the causes and effects of strikes. One chapter discusses the conditions and causes of strikes, both economic and non-economic, immediate and underlying. The diversity of conditions operating in different industries and regions of the country is well considered. The second chapter takes up the consequences of strikes. Again, attention is devoted not only to the immediate consequences for unemployment, trade union organization, subsequent character of strikes, etc., but to the long-range economic, social, and political effects as well.

The author's careful methodology in developing this book deserves note. Throughout the volume, Dr. Knowles points out various limitations which are found in strike statistics, as well as ambiguities which have developed in classification. Data for this analysis were secured by an exhaustive survey of government, trade union, and management publications. Newspaper and magazine references are cited, as well as scholarly books and articles written by British and American students of the subject. In addition, the author refers to many pamphlets and leaflets which have been distributed by trade unions, dissident political factions, rank and file groups, and political parties. The array of materials used in writing the book is indeed impressive. It is regrettable that the author was unable to employ field work to observe rank and file behavior and opinions. However, the author is aware of this limitation and emphasizes its importance.

The reviewer can report only a few of the author's findings. For example, the author concludes that increased trade union discipline has tended to restrict the size and length of strikes, while unofficial strikes or wildcats have tended to become more frequent. Workers tend to win big strikes or at least to secure favorable compromises to a greater extent than small and long strikes. A succession of one-day strikes may be far more effective than one long strike in securing concessions from employers. The author takes the position that it is impossible to assert strikes are never advisable, for concessions may be obtained in a variety of fields which are not discernible at the time of the strike. Even a lost strike may have a measure of benefit to the workers if it restrains the employer from pressing his advantage in future negotiations.

The author's discussion of the strike in a free society will interest the American reader. Dr. Knowles argues that since the strike is only one form of industrial unrest, the suppression of strikes—which is the state policy of the Soviet Union—will merely produce other sometimes more detrimental expressions of dissatisfaction, such as pegged production levels, slow-downs,

absenteeism, restrictive attitudes, etc. Hence, as long as we desire freedom of choice and competition, and other elements of our democratic society, strikes must be accepted as an integral part of our industrial life. On the other hand, improvement of communication between the rank and file and union leaders, between unions and management, and between both parties and government, and more emphasis on speedy resolution of grievances at the plant level, can reduce the length and severity of strikes.

JACK LONDON

The University of Chicago

Monopoly and Social Control. By HENRY A. WELLS. Washington, D. C.: Public Affairs Press, 1952. ix, 158 pp. \$3.25.

Mr. Wendell Berge, former Assistant Attorney General in charge of the Anti-Trust Division of the U. S. Department of Justice, thinks very well of *Monopoly and Social Control*. In an introduction to the volume he says, "I can honestly say that Mr. Wells' book . . . is the most illuminating document written in our times" (p. v). If Mr. Berge's view of the matter is at all representative, one can only conclude that we have entered upon a new Dark Age, the cultural center of which is Washington. For the light cast by this analysis of the major problem of the modern world hardly exceeds one candle power.

The welfare of a nation, according to Mr. Wells, is determined by "equitable accessibility to economic resources" (p. 1). The tendency toward the monopolization of these resources, which is everywhere and always apparent, has become especially marked in recent decades. The Russians have countered this tendency by the nationalization of the means of production and distribution, somewhat in accordance with the doctrine of Marxism. The Marxian solution to the problem of monopoly violates, of necessity, values and sentiments that are deeply embedded in Western, and particularly American, political tradition. There is little likelihood, therefore, that we will lose our freedom in the Russian manner.

But we are unwittingly wandering down another road to serfdom. Under the corporate form of economic organization, the economic resources of the nation are increasingly monopolized; the end, not far distant, is a condition of "monopoly capitalism" in which there will be the most unequal access to the nation's resources and, consequently, an appallingly low level of national welfare.

The Russian method of dealing with monopoly is unpalatable to Americans; but there is an effective means, already demonstrated, to check

the growth of monopoly capitalism that is in accord with our political traditions. This is the extension and enforcement of our existing anti-trust legislation. The great corporations will, of course, provide strong resistance. But once the majority of the American people become aware of the grave danger to their welfare of the continuing concentration of economic power in these great corporations, they will sanction and hence make effective legislative restraints on monopoly.

Such, in brief, is the major problem of our times, and its solution, as seen by Mr. Wells. The thesis is not new; Wendell Berge, David Lynch, and many others have viewed our current plight through the same small aperture. What Wells has done is to codify and systematize, and attempt to substantiate by the now rather stale findings of the Temporary National Economic Committee, the "Curse of Bigness" doctrine that evolved in Washington officialdom during the depression years.

In the Year of Our Lord 1953, *Monopoly and Social Control* is hardly more relevant than Thomas N. Carver's argument in defense of economic laissez-faire, *Essays in Social Justice*, on which this reviewer cut his intellectual eyeteeth back in the 1920's. It is raw economic determinism; it completely ignores the fact that technological and organizational exploitation of resources are far more important to a nation's welfare than simple access thereto; and it offers as a cure for corporate monopoly further aggrandizement of the biggest, if not most productive, business that this world has ever seen—the Federal Government of the United States.

RICHARD T. LAPIERE

Stanford University

Medical Public Relations: A Study of the Public Relations Program of the Academy of Medicine of Toledo and Lucas County, Ohio, 1951. By EDGAR A. SCHULER, ROBERT J. MOWITZ, and ALBERT J. MAYER. Detroit: 1952. xiv, 228 pp. No price indicated.

The subtitle identifies the material covered. This book had its genesis in postwar (1945-1950) intensification of long-developing strains within and around the social structure of professional health services in the United States. The study has received nation-wide attention through the American Medical Association as a "working solution to the biggest medical public relations problems facing the medical profession today." The authors make no such pretensions, but it does represent a considerable improvement over most such inquiries. It was conducted by qualified scientists, enjoyed academic freedom based on an independent research grant (Health Information Foundation)

to a university (Wayne), and was published.

The Lucas County society of physicians (The Academy) undertook in 1951 a program designed "to preserve the favorable attitudes toward the medical profession." The academy set up a Service Bureau (telephone exchange) to handle emergency calls for physicians; to offer "help in adjusting special problems" (complaints); and to be generally helpful in presenting the medical viewpoint to the public. This program was publicized by newspaper advertising. The authors assumed the task of observing and appraising the program.

It was admittedly impracticable to measure directly the effect on community attitudes of the Academy program, since there were no pre-program control data. Personal reactions of physicians, community leaders and the general public were obtained (by unstructured interview and structured questionnaire) after the program had been in effect about a year. The authors attempt with obvious limitations to surmise what part of the observed attitudes were attributable to the program. The public administration specialist (Mowitz) has written a lucid chapter appraising the organization of the Academy and its special program; and a second chapter describing the reactions of 50 community leaders. One sociologist (Schuler) wrote a chapter on the study of 45 physicians' attitudes and the other (Mayer) reports on the general public sample of 590 households. Some original but crude indexes of community health status and needs (vs. preferences) provide some information of interest to the sociologist concerned with the operation of health services, e.g., who carries health insurance, who has symptoms of illness, and who sees a physician where and how often, all by income and education class. The fee system turned out to be the principal focus of stress in medical public relations.

Minutiae of the investigation, including the considerations leading to each choice of method, are described. Advertising material, interviewing schedules, and field instructions are reproduced. Summary and analytical data tables in great number and a correct but awkward estimate of sampling errors are given. Modern techniques of public opinion sampling and interviewing were employed in a competent and often ingenious way. The sampling plan appears to be reasonably bias-free.

A joint chapter presents conclusions. The Lucas County program accomplished little toward solution of major medical relations problems (mostly because Academy leaders had misjudged what those problems were). Yet it should be studied and tried elsewhere. The final conclusion, "it is our conviction that

physicians *alone* [italics the reviewer's], by taking the initiative with constructive programs for such improvement, are in the best position to lead their own communities and the nation in continued progress toward better health and medical services," does not clearly follow from the data presented.

An uncommonly good public opinion survey, this study points up, by its omissions, the need for basic sociological research into the interpersonal relations inherent in medical technology (patient and physician *qua* social system), the institutional structure of health services, and the social processes underlying "a profession (that) has quite by accident fallen into a world of business and is making the adaptation which seems necessary to survival."

CARL E. HOPKINS

University of Oregon Medical School

The Scope and Method of Sociology. By PAUL HANLY FURFEY. New York: Harper and Brothers, 1953. xii, 556 pp. \$5.00.

The Scope and Method of Sociology by Paul Hanly Furfey is devoted to three major topics: the development of a new science, metasociology, which is antecedent and auxiliary to sociology; the formulation of a definition of sociology which will be consistent with the requirements of logic and the value judgments of the author; and the presentation of a variety of research methods with some evaluation of their use and desirability.

The book divides into two distinct parts of unequal value. The first deals with metasociology and the definition of sociology, the second treats with research methods. Dr. Furfey defines metasociology as a separate science from sociology. Metasociology yields criteria of scientific quality and criteria of relevance for sociology, and, in addition, offers procedural rules for the application of these criteria to sociology. While he sees the subject matter of sociology as "something existing in the real world of men and events . . . the subject matter of metasociology is sociology itself!" (p. 9). Dr. Furfey points out that while some might prefer to consider metasociology as part of the logic of science, he chooses not to accept this position. Following an extensive discussion of metasociology and some treatment of the logic of science, Dr. Furfey considers the formulation of a definition of sociology. He defines sociology as ". . . the science which seeks the broadest possible generalizations applicable to society in its structural and functional aspects" (p. 139). Having derived a definition of sociology and discussed some of the implications of his definition, Dr. Furfey turns to sociological methods.

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It is the reviewer's impression that up to this point, *The Scope and Method of Sociology* is a useful book. The concise and careful analysis of what the epistemological assumptions of sociology are and might be, and of how the definition of a discipline determines its subject matter are good reading. The reviewer feels that sociology in its present state cannot yet support a meta-sociology apart from the generalized logic of science; and, further, that other definitions of sociology than Dr. Furfey's might better serve as guides to research. As antidotes to the crude empiricism which distinguishes so much of what is called "sociology", however, Dr. Furfey views should be welcomed by all of us.

It is when he treats with research methods that Dr. Furfey's work is less successful. This may be because it is impossible adequately to discuss in any one volume the logic and techniques of methods ranging from the statistical to the use of written documents. In an attempt to scan the entire field of research methods Dr. Furfey has tried to cover too much, and at the same time, in relation to any detailed specific method, he has given too little. It is doubtful, for example, whether any student will understand sampling and its logic from the brief treatment given in this volume. The inclusion of technical formulas, as is done here, is confusing rather than enlightening to the beginner in research. Similar criticisms might be made of the other methodological chapters.

The reviewer feels that had Dr. Furfey devoted the space he gives to specific techniques of research to a fuller consideration of the logical implication of the uses of such techniques, his book would have been strengthened considerably.

Teachers in courses on the logic of research and on research methods will find *The Scope and Method of Sociology* useful collateral reading for their students. Practitioners in the sociological disciplines will profit by consideration of the problems suggested in the first half of this book.

ETHEL SHANAS

Chicago, Illinois

The Focussed Interview: A Manual (2nd edition). By ROBERT K. MERTON, MARJORIE FISKE, and PATRICIA KENDALL. New York: Bureau of Applied Social Research, Columbia University, 1952. xxv, 202 pp. \$3.00.

However much interviewing may be regarded as an art, there are probably few who would dissent from the authors' basic premise that there are techniques—"communicable and teachable procedures"—by means of which interviewing proficiency can be improved. It is wholly unfortunate, however, that the present

volume, like almost all of its predecessors, does nothing to advance these techniques beyond the level of aphoristic wisdom or of lore acquired by initiates and adepts of the field, to the status of rigorously tested knowledge of the interviewing process. Brief reference is made to the need for this undertaking when the authors describe their work as a report of "clinical experience" which has not been systematically tested, but which is to be viewed as "a first tentative step" toward the needed research enterprise (p. 4). In view of the fact that this monograph was first written in 1944 and now appears, in 1952, in its original form, without any intervening effort—apart from the cumulation of further "clinical experience"—to test its conclusions, it is difficult to take its pretensions seriously.

From the standpoint of Dr. Merton and his associates, the justification for the appearance of this volume is that it is not just another compendium of "Do's and Don'ts" for interviewers; on the one hand, it is not intended to be inclusive of all interviewing problems, and, on the other, it purports to concern itself with the unique, new and different kinds of interviewing problems encountered in the course of a rather specialized research problem. The "focussed" interview (in contrast to other kinds of research interviews, which are, apparently, not focussed on anything) is the tool which this group has developed and employed in what has come to be loosely referred to as "audience reaction research." Briefly, there is a piece of communication—a movie, a radio broadcast, an advertisement, etc.—to which the people to be interviewed have all been exposed and whose content is equally available to the interviewer in advance of the interview. The research problem which is addressed by means of the focussed interview is "to *particularize* the effective stimuli in the objective situation and to *characterize* the subject's response to it" (p. 5), or, in other words, to determine how the subject reacts to the communication and which of his reactions are linked to which elements of the communication. The volume is, thus, devoted to a rather complete specification of the kind of information such interviews should contain and the interviewing techniques which should be used to obtain it.

The interviewing problems being discussed turn out, in the final analysis, to be of essentially two kinds. First, there are, despite the authors' disclaimers, some rather pervasive matters of interviewing technique, like defining the interview situation, speaking the respondent's language, encouraging respondent spontaneity, avoiding expression of interviewer opinions, avoiding leading questions, etc. None of these problems are particularly new, and the

approach advocated in this volume with regard to each of them seems perfectly sound, in the sense that it corresponds closely with common-sense, logic, and the prevailing folklore of the field. Second, and probably of more interest, is an interrelated set of problems which derive, though the authors do not appear to recognize it, from the rigid and, in a sense, artificial restriction placed upon the interview by their particular formulation of the research goal. Here, the chronic problem becomes that of tying the opinions and feelings of the respondent to the piece of a communication he has experienced; and when, as is often the case, these are rather general opinions with a host of ramifications in to the person's experience, a kind of tug-of-war goes on in which the respondent is always drifting into talking about his opinions, while the interviewer must always struggle to pull him back to the communication, often with the result that the relevance of the one to the other appears as more manifest than real. This kind of difficulty is well-exemplified in a question which the authors quote as an example of good interviewing technique: "After seeing this film what do you think we are up against when we start invading Europe?" The chances were that the respondent, being a soldier, had a great many opinions about the invasion, quite apart from the film, and it does not seem reasonable to assume that the the interviewer's asserting or asking for a connection between the two insured that the answer necessarily bore on the connection. Even if subsequent questions asked things like: "Was there anything in the film that brought that idea out?", the respondent's ability to cite elements from the film in support of his opinion does not appear to be conclusive evidence about either his reactions to the film or the film's influence on him. The problems implicit in securing valid reports, retrospectively, of people's feeling during some past experience are worthy of more attention than are here given them, and seem unlikely to yield to mechanical manipulation of the wording of questions.

In summary, then, while much of what is said about interviewing technique represents good current practice, there are points on which other experienced practitioners in the field would disagree, and no method other than opinion and experience is offered for the resolution of these disagreements about technique. Much of this material may, however, be found more conveniently in other sources, since this book is concerned with a field of research with which few are familiar and require, therefore, the ability to translate what is being said to more familiar research situations in order to evaluate

it fully. It is most challenging if it is read as a manual of a particular research project; that is, if it is regarded as a descriptive statement of how Dr. Merton and his associates addressed themselves to a particular research problem they had, how they came to define their objectives, and what techniques they developed to fit these objectives, or, even, what standard instructions they issued to their interviewing staff. As the case history of a research carried out by some of our most skilled technicians, it is well worth attention, even though it is incomplete in the sense that the very difficult problem of systematizing and analyzing this kind of free-flowing interview materials is postponed for treatment in a later work. Once again, however, the interested reader will have to translate freely to obtain a research history from this document, for its tone is not so much that of explaining what was, historically, done as it is one of dogmatically declaring what must, inevitably, be done.

SHIRLEY A. STAR

National Opinion Research Center,
University of Chicago

Modern Elementary Statistics. By JOHN E. FREUND. New York: Prentice-Hall, Inc., 1952. x, 418 pp. \$5.50.

Although this book was apparently written for a general introductory course in statistics within mathematics departments, it appears to be equally usable for courses in social statistics. In many ways it is a definite improvement over the available textbooks in beginning statistics written for sociology students or for students in other social science departments.

First, it does not avoid the use of necessary mathematical notations, operations, and theorems, but keep them on levels which are easily understood and usable by students acquainted only with elementary algebra. Second, it clearly distinguishes between empirical and theoretical frequency distributions, and avoids the over-emphasis and idealization of the normal curve that is common in other elementary texts. Third, it does not restrict itself to the discussion of elementary formulae, but includes a discussion of statistical theories and methods involved in sampling, estimation, testing hypotheses, and prediction that are essential to social research. Treatment of these subjects, however, is on a level which can be understood even by beginning students, both in terms of the mathematical analysis and the examples of the application in various subject matter fields. Fourth, the treatment of index numbers and time series is properly dealt with as special problems and are

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adequately treated without undue emphasis. And fifth, an attempt is made to treat the logical and scientific aspects of statistics as well as the purely mathematical and computational.

There are a number of limitations to be noted in spite of the general superiority of this text. First, although in general it avoids the use of statistical formulae without adequate explanation of the assumptions underlying their use, occasionally there are lapses from this excellent principle. For example, on page 12 a formula is given to determine the number of classes in frequency distributions without justifying its preference over more obvious rules. Also, in the discussions of tests of hypotheses, assuming the "Normal Distribution" or the "Student's *t* Distribution," the conditions other than random sampling required to justify the assumption of these distributions are not stated. Second, an excellent beginning is made in the discussion of the problem of prediction and in the interpretation of correlation methods in terms of prediction theory. But as the discussion proceeds, the treatment becomes more and more conventional. This is particularly true with respect to the treatment of the correlation of qualitative data, where the author might well have made use of Guttman's "Principle of Maximum Probability" which was developed for the prediction of qualitatively defined variables. Third, the "Chi-square criterion" is discussed in detail mainly with regard to the problem of correlation rather than under the problem of testing hypotheses, where it more appropriately belongs. Fourth, aside from the discussion of index numbers, the problems of measurement and scaling are not given consideration. Fifth, although the theory of testing hypotheses is clearly presented, an expedient is used in order to avoid having to consider the "Errors of type II." This expedient is to assume that if the hypothesis is not rejected, the alternative is to "reserve our judgment." This may solve the problem linguistically, but in practice it is obvious that unless the hypothesis is rejected or unless the decision is to continue experimentation, then the alternative action is to accept the hypothesis as true. Also, even though the probability of error of type II may be difficult to compute, it is useful to evaluate the efficiency of various types of experimental design.

The author has kept his treatment to about 400 pages, including tables and appendices, by virtue of a conciseness of statement which should appeal to those who object to the wordy and repetitious discussion often found in texts written for social science students.

JULIUS A. JAHN

The State College of Washington

An Approach to Measuring Results in Social Work. By DAVID G. FRENCH. New York: Columbia University Press, 1952. xiv, 178 pp. \$3.00.

America spends an estimated two-and-a-half billion dollars annually on its social welfare services. By comparison the amount spent on research to evaluate the effectiveness of these services is virtually zero. Dissatisfaction over this anomaly by the supporters of and the practitioners in social services constitutes the background of the work under review. This book, consisting of seven chapters and two appendices, is not an actual piece of evaluative research. It is rather, in the author's words, a reconnaissance, which surveys the relevant questions, identifies the obstacles, points out the approaches and suggests a definite plan for a continuing program of evaluative research in social work. The author argues for an institute for social work research attached to a professional school of social work in a university, staffed by an interdisciplinary team of social scientists and social workers, where basic problems of practice would be approached from a social science theory orientation. He pursues his argument with simplicity, conciseness and a genuine appreciation of fundamental issues.

The first two chapters of the volume are introductory and describe the reasons for the author's undertaking which was financed by the Michigan Welfare League. In gathering material for the book the author conducted a series of conferences attended by social work practitioners, administrators, educators and donors, out of which there emerged well over a hundred questions about social work programs. Presenting the full list of questions in an appendix, the author devotes Chapter III to their systematization into six broad classes and discusses them fully, developing their implications. Thereby he shrewdly highlights a number of basic theoretical problems whose prior solution is a prerequisite for any successful research on social work questions. Chapter IV is the best exposition this reviewer has anywhere seen of the logic of evaluative research. To pass muster, every adequate specimen of evaluative research must, according to our author, answer five desiderata. He submitted to each of four competent research consultants an important social work study and requested them to analyze these studies in terms of his five criteria. Relegating the four critiques to a thirty-page appendix, the author draws upon them heavily to illustrate the manifold obstacles of a methodological and substantive nature which any successful program of evaluative research in social work must first surmount.

It is the contention of Chapter V that the problems attached to the pursuit of evaluative research by the social work profession are soluble only through the joint effort of social work and social science in the form either of persons who have been trained in both disciplines or of teams representing the two fields. Research problems emerging out of social work practice must be so formulated that they become related to the body of theory of the social sciences, thereby standing to benefit from the latter. Reciprocally, social work practice offers to social scientists possibilities for experimental verification, and hence expansion, of their theory. In this manner the author builds up his case for a university-based institute of social work research staffed by an interdisciplinary team. He devotes Chapter VI to justifying the location of the institute in the school of social work rather than in any of the university social science departments or in an operating social agency. The final chapter contains a description of the suggested personnel, administration, budget and financing of the proposed research institute.

The central thesis of this book will probably not meet the warmest of reception among the entire social work profession. We refer to those who feel that students trained in the pure social sciences do not understand the goals, techniques and setting of social work practice and, aside from a few technical research tricks, have little to offer toward the solution of the practitioner's problems; so that if social work is to have a profession-sponsored program of evaluative research, it should be "owned and operated" exclusively by social workers. In this respect the author disagrees with some of his professional colleagues. Mr. French has written a readable, illuminating, non-technical book which performs well the reconnoitering operation he has set for himself. It remains to be seen whether the social work profession will utilize the blue-print he has produced.

ERNEST GREENWOOD

University of Pittsburgh

The Volunteer Work Camp: A Psychological Evaluation. By HENRY W. RIECKEN. Cambridge, Mass.: Addison-Wesley Press, 1952. xviii, 262 pp. \$3.50.

This is by far the most successful scientific appraisal of summer programs set up to affect social attitudes and behavior. The research design called for (1) a pretest of all participants in American Friends' Service Committee volunteer work camps for the summer of 1948 with a Sentiments Inventory, (2) a second test of the

same group at the end of the camp period, (3) a final administration of the inventory to the campers ten months after the closing of the camps (to measure the stability of attitude changes). In addition, two camps were studied more intensively by participant observers who, in addition to their notes, administered additional tests: the Allport-Vernon, the Thematic Apperception Test, A Sentence-Completion Test of a projective character, and a Guess-Who Test. Though the number of campers who completed all three forms of the inventory was only about 29 per cent of the total, Riecken shows by means of internal evidence that it was a representative sample (pp. 41-42). Copies of the instruments are included in the Appendix, along with the scores (the most important, Part III, includes the familiar short forms of the Ethnocentrism, Authoritarianism, and Political-Economic Conservatism scales from the *Authoritarian Personality* plus items on Nonviolence, Democracy, and Social Class Axioms).

The detailed findings are decidedly worth study. A few worth mentioning are (1) significant long run changes toward less authoritarian, less ethnocentric and more democratic attitudes; (2) short-run changes toward more pacifist positions and reversion toward non-pacifist attitudes in ten months; (3) no significant change in political-economic conservatism or in social-class "axioms" in the long run; (4) those who showed more attitude changes (as given in 1 above) were those whose previous views were somewhat romanticized, and those who had more contact with their fellow campers the following year; (5) campers feel more alienated from the "average man" on many of the scales than a similar sample of Harvard and Radcliffe students; (6) from one-third to one-fourth of the campers were definitely influenced in their choice of vocation by the camp experience.

It would be possible to single out other significant results but perhaps none of greater importance. Riecken's discussion of his methods is straightforward and direct; he recognizes the weakness in having no control group, in the unreliability of his Social Class Axioms Scale, and the small sample. Perhaps the least satisfactory of his findings has to do with behavioral change which is wholly inferential (pp. 142-145). The sociologist would certainly prefer to categorize the campers more extensively so as to correlate attitude changes with religious affiliation, racial status, urban or rural residence, types of college attended (small denominational, state, large private, etc.), family relationships, etc. The fact remains that Riecken has added a solid contribution to the many evaluation studies in cognate

fields, and one which serves as a useful guide for all future research of similar nature. Especially helpful are his suggestions for increasing the effectiveness of programs in the summary chapter: for example he recommends that efforts be made to obtain several campers from each local group (in this case, the college) so that the reinforcement of the group can maintain the configuration of attitudes once established.

In addition to the body of the text, there is a preface by the Advisory Committee of the evaluation project: G. W. Allport, Jerome S. Bruner, Stephen G. Cary, David C. McClelland, Ira de A. Reid and Fillmore Sanford. The Appendix also includes "An Evaluation of AFSC Volunteer Work Service Camps in Germany" by G. W. Allport, made in 1948, a report of both historic and scientific interest in spite of its brevity. The volume ends with a selected bibliography, but alas, no index.

R. A. SCHERMERHORN

Western Reserve University

Social and Psychological Factors in Opiate Addiction: A Review of Research Findings Together with an Annotated Bibliography. Edited by ALAN S. MEYER. New York: Bureau of Applied Social Research, Columbia University, 1952. ix, 170 pp. \$1.00.

Revived interest in the problems of opiate addiction in recent years has called forth a great volume of popular literature, much of it sensational and controversial. Many contradictory assertions have a semblance of reliability based on the serious publications in the field. This survey may be called a "cold-water" treatment of the current evidence and may exert a wholesome astringent influence as far as its effects are felt.

To quote from the prefatory remarks: "The purpose of the present work is threefold. . . . To make available an annotated bibliography of literature published since 1928 which bears on social and psychological factors in opiate addiction. . . . To present the results of research studies, summarized within a framework of these social and psychological factors. . . . To call attention to gaps in present knowledge and suggest problem areas suitable for further sociological and psychological research."

The editor has denied himself the luxury of comment or independent hypotheses and has summarized the findings of others in this review conducted for the National Institute of Mental Health of the United States Public Health Service. Section I (pp. 1-124) is a review of 56 research findings, examined under ten headings, "Extent of Drug Addiction," "Patterns of Drug

Use," "Characteristics of Drug Users," "Concomitants of Drug Use," "Factors Leading to Initial Use," "Causes of Addiction," "Factors Leading to Treatment," "Roads to Relapse," "Roads to Abstinence," and "Prevention of Addiction."

Section II (pp. 125-170) contains 159 annotated bibliographical items on opiate addiction and two pages of 33 entries on marihuana without annotations. Foreign sources have been omitted from this survey but the hope is expressed that comparative studies may be conducted later. The entire presentation is closely knit with cross references and the product is an accessible source work. Students of social pathology who have labored through the scattered literature of opiate addiction will find this work an instrument of great help.

The deficiencies of data on opiate addiction are painfully apparent from an examination of the findings summarized in this report. In many cases the conclusions of statistical studies are quite contradictory. Few investigators have been able to secure large samples and who can say that any one of them is representative? Local studies are usually directed at special groups who have proved annoying. The more comprehensive investigations have been conducted with the inmates of federal institutions but are not necessarily representative of addiction for the nation. With all credit to the courageous pioneers in this field, the returns of their reports show little consistency or continuity. It is difficult to reach any conclusions concerning the extent of addiction, much less the differentials for regions, social classes, occupations, or ethnic groups. This inventory implies many areas for more intensive investigation but there is a serious question as to whether there are cores of reliable data from which subsequent studies can select points of reference. Social action appears to have been based very largely on popular concepts of drug addiction and it is entirely probable that no alternative will be available for some time to come.

The editor has done his work faithfully and painstakingly. If he has revealed any bias it is not apparent to the reviewer. The report might include a definition of opiate addiction without going beyond the scope of its objectives, especially since there are references to distinctions between addiction and "habituation." Although they may lie outside the scope of this publication, it is to be hoped that the editor and his associates will be able to formulate and give to future investigators some specific issues for profitable investigation. It would also be helpful to

have a summary of the principal projects currently under way in this field. These are not necessarily the goals or responsibilities of the present work, but they are logical by-products of the labors performed in assembling the literature

and should contribute something to the validity of new enterprises in the area.

W. WALLACE WEAVER

University of Pennsylvania

PUBLICATIONS RECEIVED

(Listing of a publication below does not preclude its subsequent review)

- Amerika-Mura: Imin Soshutzu Son no Jittae (Influences of Emigrants on Their Home Village: Report of a Survey of "Amerika-Mura")*. Tokyo: Tokyo University Press, 1953. \$2.20 plus 50 cents mailing charge.
- ANDERSON, W. A. *Some Factors Associated with Family Informal Participation*. Ithaca, New York: Cornell University Agricultural Experiment Station, Department of Rural Sociology, Mimeograph Bulletin No. 36, 1953. 31 pp. No price indicated.
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- BLUM, GERALD S. *Psychoanalytic Theories of Personality*. New York: McGraw-Hill Book Company, Inc., 1953. xviii, 219 pp. \$3.75.
- BONNER, HUBERT. *Social Psychology: An Interdisciplinary Approach*. New York: American Book Company, 1953. 439 pp. \$4.25.
- BREWSTER, PAUL. *American Nonsinging Games*. Norman: University of Oklahoma Press, 1953. xxii, 218 pp. \$3.75.
- BROWN, JAMES S. *The Family Group in a Kentucky Mountain Farming Community*. Lexington: Kentucky Agricultural Experiment Station, University of Kentucky, June 1952 (Bulletin 588). 38 pp. No price indicated.
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- BUROS, OSCAR KRISEN (Editor). *Classified Index of Tests and Reviews in the Fourth Mental Measurements Yearbook*. Highland Park, New Jersey: The Gryphon Press, 1953. No price indicated.
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- DRYBURGH, GEORGE M. (Chairman). *The Church Under Communism* (Second Report of the Commission on Communism appointed by General Assembly of the Church of Scotland). New York: Philosophical Library, 1953. 79 pp. \$2.75.
- DUVALL, EVELYN MILLIS and REUBEN HILL. *When You Marry* (Revised Edition). New York: D. C. Heath and Company, 1953. xii, 466 pp. \$3.50.
- ERASMUS, CHARLES J. *Las Dimensiones de la Cultura: Historia de la Etnologia en los Estados Unidos Entre 1900 y 1950*. Bogota: Editorial Iqueima, 1953 (obtainable in United States through O'Brien Linotyping Co., 14931 Burbank Blvd., Van Nuys, California). vii, 198 pp. \$2.00.
- FINKELSTEIN, LOUIS (Editor). *Thirteen Americans: Their Spiritual Autobiographies*. New York: The Institute for Religious and Social Studies (distributed by Harper and Brothers), 1953. xii, 297 pp. \$3.00.
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- GOEDICKE, VICTOR. *Introduction to the Theory of Statistics*. New York: Harper and Brothers, 1953. xii, 286 pp. \$5.00.
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- HAYNES, WILLIAM WARREN. *Nationalization in Practice: The British Coal Industry*. Boston: Division of Research, Graduate School of Business Administration, Harvard University (printed at Riverside Press, Cambridge), 1953. xviii, 413 pp. \$4.00.
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- HOLLAND, WILLIAM L. (Editor). *Asian Nationalism and the West: A Symposium Based on Documents and Reports of the Eleventh Conference, Institute of Pacific Relations*. New York: The Macmillan Company, 1953. viii, 449 pp. \$5.00.
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- (HOUSE OF REPRESENTATIVES, 82ND CONGRESS, 2ND SESSION (Report No. 2514)). *Final Report of the Select Committee to Investigate Foundations and Other Organizations (Pursuant to H. Res. 561, 82nd Congress)*. Washington, D. C.: Government Printing Office, 1953. 15 pp. No price indicated.
- (HOUSING AND HOME FINANCE AGENCY) (Division of Housing Research). *Housing of the Non-white Population 1940-1950*. Washington, D. C.: Government Printing Office, 1952. 42 pp. Twenty-five cents.
- KATZ, BARNEY and GEORGE LEHNER. *Mental Hygiene in Modern Living*. New York: The Ronald Press Company, 1953. x, 544 pp. \$4.50.
- KENT, DONALD PETERSON. *The Refugee Intellectual: The Americanization of the Immigrants of 1933-1941*. New York: Columbia University Press, 1953. xx, 317 pp. \$5.00.
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